PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXI.

PART I.

LONDON,

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MDCCCXXI.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

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be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

Meteorological Journal for 1820, *kept at the Apartments of the Royal Society.*

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1820, to Professor JOHN CHRISTIAN OERSTED, of Copenhagen, for his Electro-magnetic Discoveries.

PHILOSOPHICAL TRANSACTIONS.

I. *On the black rete mucosum of the Negro, being a defence against the scorching effect of the sun's rays.* By Sir Everard Home, *Bart. F. R. S.*

Read November 9, 1820.

To ascertain the use of the black colour of the rete mucosum in the Negro, has occupied the attention of many physiologists; and I confess that this subject formed the first investigation in which I ever engaged. Fruitless, indeed, were my attempts; and when I learnt that black surfaces absorbed heat, and raised the temperature several degrees beyond any others, I gave the matter up in despair. Two years ago my attention was again called to this enquiry, upon being told by our late excellent President, that a silver fish, in a pond at Spring Grove, during a very hot summer, immediately after some trees by which the pond was shaded were cut down, was so much exposed to the sun's rays as to have its back scorched, the surface putting on the same appearance as after a burn, and rising above the scales of the

surrounding skin. I saw the fish several times, and directions were given to send it to me when it died; but I was not so fortunate as to receive it.

This extraordinary circumstance brought to my recollection one not less so. In crossing the Tropic in April, 1781, at twelve o'clock at noon, in a voyage to the West Indies, I had fallen asleep upon deck, lying upon my back, having a thin linen pair of trowsers on, and I had not slept half an hour, when I was awakened by the bustle attending the demand of forfeits on crossing the Line, and found the inside of the upper part of both thighs scorched, the effects of which have never gone off, but till now I could not imagine how it happened, always suspecting it to be the effect of the bites of insects; but I never satisfied myself upon that subject.

The effect of the sun's rays upon the fish under water, led me to suspect the mixture of light and heat to be the cause of this scorching effect.

To ascertain the truth of this opinion, I made the following experiments.

Experiment 1.

In August, 1820, I exposed the back of my hand to the sun at twelve o'clock, with a thermometer attached to it, another thermometer being placed upon a table, with the same exposure. That on my hand stood at 90°, the other at 102°. In 45 minutes blisters rose, and coagulable lymph was exuded, which became vascular under my eye: the pain was very severe.

Experiment 2.

I exposed my face, my eyelids, and the back of my hand to water heated to 120°: in a few minutes they became pain-

ful; and when the heat was further increased, I could not bear it.

Experiment 3.

I exposed the backs of my two hands to the sun's rays, with a thermometer upon each; the one hand was uncovered, the other had a covering of black cloth, under which the ball of the thermometer was placed. After ten minutes, the degree of heat of each thermometer was marked, and the appearance on the skin examined. This was repeated at three different times. The

1st time	the thermometer under the cloth	91°, the other 85°
2nd time	- - - - -	94 - - 91
3rd time	- - - - -	106 - - 98

In every one of these trials the skin was scorched that was uncovered; the other had not suffered in the slightest degree; there was no appearance of perspiration on either hand.

Experiment 4.

The back of a Negro's hand was exposed to the sun with a thermometer upon it, which stood at 100°; at the end of ten minutes the skin had not suffered in the least.

Experiment 5.

During the eclipse of the sun on September 7, 1820, I exposed the back of my hand to the rays concentrated by a double lens of half an inch focus, at three different periods of the eclipse. When the heat to a thermometer was 75°, that is from 47 to 57 minutes past one o'clock, (including three of the figures in the annexed drawing, made by Mr. Bauer, Pl. I.)

the concentrated rays felt warm, but gave no pain, although applied for ten minutes.

When the heat to a thermometer was 79°, that is at 15 minutes past two o'clock (including the twelfth figure in the annexed drawing), the concentrated rays in four minutes gave pain; in five minutes blistered the skin, and produced dots of coagulable lymph, which became vascular under the eye.

When the heat to a thermometer was 82°, that is at half past two o'clock, (including the 13th figure of the drawing), the concentrated rays in three minutes gave pain; in four, the part was blistered, and the pain could not longer be endured.

Experiment 6.

September 8th, 1820, at eleven o'clock, the heat in the sun 90°; the concentrated rays applied to my naked arm produced a vesicle. This experiment was repeated when the heat was 84°, and in seven minutes a blister formed on the arm.

Experiment 7.

September 9th, eleven o'clock, the thermometer in the sun at 90°. The concentrated rays applied to a piece of black kerseymere cloth, made tight round my arm for 15 minutes, gave no real pain, and left no impression whatever on the skin, although the nap of the cloth had been destroyed.

This experiment was repeated with white kerseymere, the heat at 86°; in 15 minutes a blister was formed.

Repeated with Irish linen, the thermomenter 86°. In 15 minutes a blister was formed, and coagulable lymph thrown out, which had become vascular.

The same experiment was made with a white handkerchief loose upon the hand, the heat 88°. In 15 minutes an inflammatory blush was produced over a surface of several inches extent, which almost immediately disappeared on withdrawing the hand from the sun's rays.

Experiment 8.

September 19th. The sun's heat at noon 85°. The concentrated rays applied to the back of the hand of a Negro from Grenada for 15 minutes, produced no visible effect; at the first moment he felt a stab going inwards, but that went off, and afterwards he had no pain.

From these experiments, it is evident that the power of the sun's rays to scorch the skin of animals is destroyed when applied to a black surface, although the absolute heat, in consequence of the absorption of the rays, is greater.

The same wise providence which has given so extraordinary a provision to the Negro for the defence of his skin, while living within the tropics, has extended it to the bottom of the eye, which otherwise would suffer in a greater or less degree when exposed to strong light; the retina, from its transparency, allowing it to pass through without injury.

That the nigrum pigmentum is not necessary for vision, but only provided as a defence against strong light, is proved by its being darker in the Negro than the European, and being of a lighter colour in fair people than in dark, and therefore lightest in those countries farthest removed from the effects of the sun.

In the monkey it is dark, and in all animals that look upwards.

In all birds exposed to the sun's rays the nigrum

pigmentum is black. In fishes, the basking shark, which lies upon the surface of the ocean, has a nigrum pigmentum. The turbot and skate, which lie upon banks of sand in shallow water, have nigrum pigmentum.

In all ruminating animals and birds of prey, there is a lucid tapetum at the bottom of the eye.

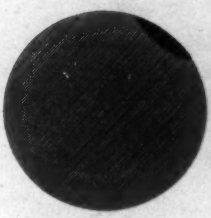
The owl, that never sees the sun, has no nigrum pigmentum.

The mackarel has the bottom of the eye lucid as quicksilver.

The *Coup de Soleil*, met with in the West Indies, the effects of which I have seen, I attribute to the scorching effect of the sun's rays upon the scalp.

The Egyptian ophthalmia I consider to be the effect of the sun's rays, and the glare of reflected light.

I have stated the fact of the scorching power of the sun's rays being destroyed when they are applied to black surfaces, but have not gone farther. Sir HUMPHRY DAVY, to whom I showed these observations, immediately explained it. He said the radiant heat in the sun's rays was absorbed by the black surface, and converted into sensible heat.



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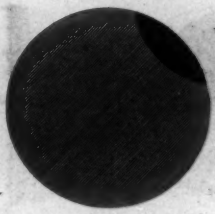

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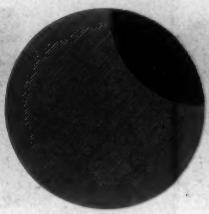

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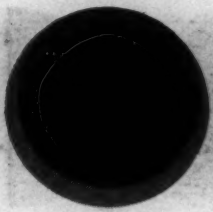

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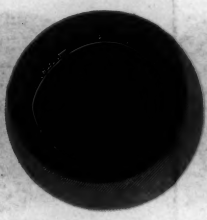

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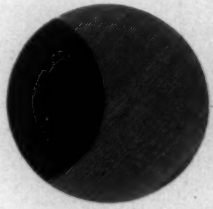

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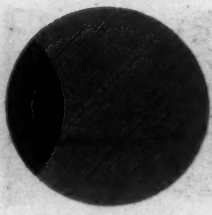

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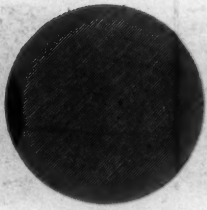

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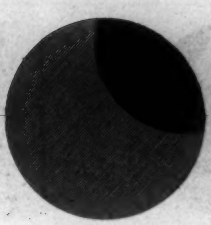

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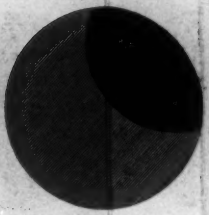

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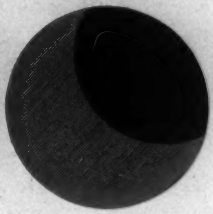

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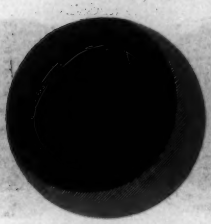

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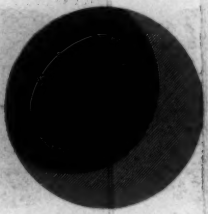

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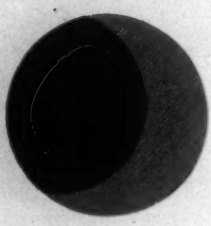

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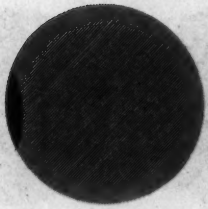

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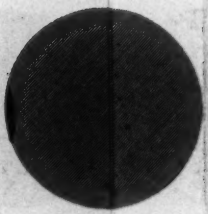

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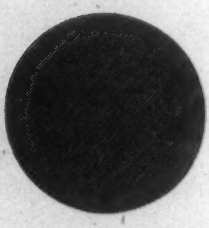

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II. *On the magnetic phenomena produced by Electricity; in a letter from Sir* H. Davy, *Bart. F. R. S. to* W. H. Wollaston, *M. D. P. R. S.*

Read November 16, 1820.

My dear Sir,

The similarity of the laws of electrical and magnetic attraction has often impressed philosophers; and many years ago, in the progress of the discoveries made with the voltaic pile, some enquirers (particularly M. Ritter,*) attempted to establish the existence of an identity or intimate relation between these two powers; but their views being generally

* M. Ritter asserted that a needle composed of silver and zinc arranged itself in the magnetic meridian, and was slightly attracted and repelled by the poles of a magnet; and that a metallic wire, after being exposed in the voltaic circuit, took a direction N. E. and S. E. His ideas are so obscure, that it is often difficult to understand them; but he seems to have had some vague notion that electrical combinations, when not exhibiting their electrical tension, were in a magnetic state, and that there was a kind of electro-magnetic meridian depending upon the electricity of the earth. See Annales de Chimie, T. 64, p. 80. Since this letter has been written, Dr. Marcet has been so good as to send me from Genoa, some pages of Aldini on Galvanism, and of Izarn's Manual of Galvanism, published at Paris more than sixteen years ago. M. Mojon, senior, of Genoa, is quoted in these pages as having rendered a steel needle magnetic, by placing it in a voltaic circuit for a great length of time. This, however, seems to have been dependent merely upon its place in the magnetic meridian, or upon an accidental curvature of it; but M. Romagnesi, of Trente, is stated to have discovered that the pile of Volta caused a declination of the needle; the details are not given, but if the general statement be correct, the author could not have observed the same fact as M. Oersted, but merely supposed that the needle had its magnetic poles altered after being placed in the voltaic circuit as a part of the electrical combination.

obscure, or their experiments inaccurate, they were neglected: the chemical and electrical phenomena exhibited by the wonderful combination of Volta, at that time almost entirely absorbed the attention of scientific men; and the discovery of the fact of the true connection between electricity and magnetism, seems to have been reserved for M. OERSTED, and for the present year.

This discovery, from its importance and unexpected nature, cannot fail to awaken a strong interest in the scientific world; and it opens a new field of enquiry, into which many experimenters will undoubtedly enter: and where there are so many objects of research obvious, it is scarcely possible that similar facts should not be observed by different persons. The progress of science is, however, always promoted by a speedy publication of experiments; hence, though it is probable that the phenomena which I have observed may have been discovered before, or at the same time in other parts of Europe, yet I shall not hesitate to communicate them to you, and through you to the Royal Society.

I found, in repeating the experiments of M. OERSTED with a voltaic apparatus of one hundred pair of plates of four inches, that the south pole of a common magnetic needle (suspended in the usual way) placed under the communicating wire of platinum, (the positive end of the apparatus being on the right hand) was strongly attracted by the wire, and remained in contact with it, so as entirely to alter the direction of the needle, and to overcome the magnetism of the earth. This I could only explain by supposing that the wire itself became magnetic during the passage of the electricity through it, and direct experiments, which I immediately made, proved that this was the case. I threw

some iron filings on a paper, and brought them near the communicating wire, when immediately they were attracted by the wire, and adhered to it in considerable quantities, forming a mass round it ten or twelve times the thickness of the wire: on breaking the communication, they instantly fell off, proving that the magnetic effect depended entirely on the passage of the electricity through the wire. I tried the same experiment on different parts of the wire, which was seven or eight feet in length, and about the twentieth of an inch in diameter, and I found that the iron filings were every where attracted by it; and making the communication with wires between different parts of the battery, I found that iron filings were attracted, and the magnetic needle affected in every part of the circuit.

It was easy to imagine that such magnetic effects could not be exhibited by the electrified wire without being capable of permanent communication to steel. I fastened several steel needles, in different directions, by fine silver wire to a wire of the same metal, of about the thirtieth of an inch in thickness and eleven inches long, some parallel, others transverse, above and below in different directions; and I placed them in the electrical circuit of a battery of thirty pairs of plates of nine inches by five, and tried their magnetism by means of iron filings: they were all magnetic: those which were parallel to the wire attracted filings in the same way as the wire itself, but those in transverse directions exhibited each two poles, which being examined by the test of delicate magnets, it was found that all the needles that were placed under the wire (the positive end of the battery being east) had their north poles on the south side of the wire, and their

south poles on the north side; and that those placed over, had their south poles turned to the south, and their north poles turned to the north; and this was the case whatever was the inclination of the needles to the horizon. On breaking the connection, all the steel needles that were on the wire in a transverse direction retained their magnetism, which was as powerful as ever, whilst those which were parallel to the silver wire appeared to lose it at the same time as the wire itself.

I attached small longitudinal portions of wires of platinum, silver, tin, iron, and steel, in tranverse directions, to a wire of platinum that was placed in the circuit of the same battery. The steel and the iron wire immediately acquired poles in the same manner as in the last experiment; the other wires seemed to have no effect, except in acting merely as parts of the electrical circuit; the steel retained its magnetism as powerfully after the circuit was broken as before; the iron wire immediately lost a part of its polarity, and in a very short time the whole of it.

The battery was placed in different directions as to the poles of the earth; but the effect was uniformly the same. All needles placed transversely under the communicating wires, the positive end being on the right hand, had their north poles turned towards the face of the operator, and those above the wire their south poles; and on turning the wire round to the other side of the battery, it being in a longitudinal direction, and marking the side of the wire, the same side was always found to possess the same magnetism; so that in all arrangements of needles transversely round the wire, all the needles above had north and south poles oppo-

site to those below, and those arranged vertically on one side, opposite to those arranged vertically on the other side.

I found that contact of the steel needles was not necessary, and that the effect was produced instantaneously by the mere juxta-position of the needle in a transverse direction, and that through very thick plates of glass: and a needle that had been placed in a transverse direction to the wire merely for an instant, was found as powerful a magnet as one that had been long in communication with it.

I placed some silver wire of $\frac{5}{10}$ of an inch and some of $\frac{1}{10}$, in different parts of the voltaic circuit when it was completed, and shook some steel filings on a glass plate above them: the steel filings arranged themselves in right lines always at right angles to the axis of the wire; the effect was observed, though feebly, at the distance of a quarter of an inch above the thin wire, and the arrangement in lines was nearly to the same length on each side of the wire.

I ascertained by several experiments, that the effect was proportional to the quantity of electricity passing through a given space, without any relation to the metal transmitting it: thus, the finer the wires the stronger their magnetism.

A zinc plate of a foot long and six inches wide arranged with a copper plate on each side, was connected, by a very fine wire of platinum, according to your method; and the plates were plunged an inch deep in diluted nitric acid. The wire did not sensibly attract fine steel filings. When they were plunged two inches, the effect was sensible; and it increased with the quantity of immersion. Two arrangements of this kind acted more powerfully than one; but when the two were combined so as to make the zinc and copper-plates but

parts of one combination, the effect was very much greater. This was shown still more distinctly in the following experiment. Sixty zinc plates with double copper-plates were arranged in alternate order, and the quantity of iron filings which a wire of a determinate thickness took up observed: the wire remaining the same, they were arranged so as to make a series of thirty; the magnetic effect appeared more than twice as great; that is, the wire raised more than double the quantity of iron filings.

The magnetism produced by voltaic electricity seems (the wire transmitting it remaining the same) exactly in the same ratio as the heat; and however great the heat of a wire, its magnetic powers were not impaired. This was distinctly shown in transmitting the electricity of twelve batteries of ten plates each of zinc, with double copper arranged as three, through fine platinum wire, which when so intensely ignited as to be near the point of fusion, exhibited the strongest magnetic effects, and attracted large quantities of iron filings and even small steel needles from a considerable distance.

As the discharge of a considerable quantity of electricity through a wire seemed necessary to produce magnetism, it appeared probable, that a wire electrified by the common machine would not occasion a sensible effect; and this I found was the case, on placing very small needles across a fine wire connected with a prime conductor of a powerful machine and the earth. But as a momentary exposure in a powerful electrical circuit was sufficient to give permanent polarity to steel, it appeared equally obvious, that needles placed transversely to a wire at the time that the electricity of a common Leyden battery was discharged through it,

ought to become magnetic; and this I found was actually the case, and according to precisely the same laws as in the voltaic circuit; the needle *under* the wire, the positive conductor being on the right hand, offering its north pole to the face of the operator, and the needle *above*, exhibiting the opposite polarity.

So powerful was the magnetism produced by the discharge of an electrical battery of 17 square feet highly charged, through a silver wire of $\frac{1}{20}$ of an inch, that it rendered bars of steel of two inches long and from $\frac{1}{20}$ to $\frac{1}{10}$ in thickness, so magnetic, as to enable them to attract small pieces of steel wire or needles; and the effect was communicated to a distance of five inches above or below or laterally from the wire, through water or thick plates of glass or metal electrically insulated.

The facility with which experiments were made with the common Leyden battery, enabled me to ascertain several circumstances which were easy to imagine, such as that a tube filled with sulphuric acid of $\frac{1}{4}$ of an inch in diameter, did not transmit sufficient electricity to render steel magnetic; that a needle placed transverse to the explosion through air, was less magnetized than when the electricity was passed through wire; that steel bars exhibited no polarity (at least at their extremities) when the discharge was made through them as part of the circuit, or when they were placed parallel to the discharging wire; that two bars of steel fastened together, and having the discharging wire placed through their common centre of gravity, showed little or no signs of magnetism after the discharge till they were separated, when they exhibited their north and south poles opposite to each other, according to the law of position.

These experiments distinctly showed, that magnetism was produced whenever concentrated electricity passed through space; but the precise circumstances, or law of its production, were not obvious from them. When a magnet is made to act on steel filings, these filings arrange themselves in curves round the poles, but diverge in right lines; and in their adherence to each other form right lines, appearing as spicula. In the attraction of the filings round the wire in the voltaic circuit, on the contrary, they form one coherent mass, which would probably be perfectly cylindrical were it not for the influence of gravity. In first considering the subject, it appeared to me that there must be as many double poles as there could be imagined points of contact round the wire; but when I found the N. and S. poles of a needle uniformly attracted by the same quarters of the wire, it appeared to me that there must be four principal poles corresponding to these four quarters. You, however, pointed out to me that there was nothing definite in the poles, and mentioned your idea, that the phenomena might be explained, by supposing a kind of revolution of magnetism round the axis of the wire, depending for its direction upon the position of the negative and positive sides of the electrical apparatus.

To gain some light upon this matter, and to ascertain correctly the relations of the north and south poles of steel magnetized by electricity to the positive and negative state, I placed short steel needles round a circle made on paste-board, of about two inches and half in diameter, bringing them near each other, though not in contact, and fastening them to the paste-board by thread, so that they formed the sides of a hexagon inscribed within the circle. A wire was

fixed in the centre of this circle, so that the circle was parallel to the horizon, and an electric shock was passed through the wire, its upper part being connected with the positive side of a battery, and its lower part with the negative. After the shock all the wires were found magnetic, and each had two poles; the south pole being opposite to the north pole of the wire next to it, and vice versa; and when the north pole of a needle was touched with a wire, and that wire moved round the circle to the south pole of the same needle, its motion was opposite to that of the apparent motion of the sun.

A similar experiment was tried with six needles arranged in the same manner; with only this difference, that the wire positively electrified was below. In this case the results were precisely the same, except that the poles were reversed; and any body, moved in the circle from the north to the south pole of the same needle, had its direction from east to west.

A number of needles were arranged as polygons in different circles round the same piece of paste-board, and made magnetic by electricity; and it was found that in all of them, whatever was the direction of the paste-board, whether horizontal or perpendicular, or inclined to the horizon, and whatever was the direction of the wire with respect to the magnetic meridian, the same law prevailed; for instance, when the positive wire was east, and a body was moved round the circle from the north to the south poles of the same wire; its motion (beginning with the lower part of the circle) was from north to south, or with the upper part from south to north; and when the needles were arranged round a cylinder of paste-board so as to cross the wire, and a pencil mark drawn in the direction of the poles, it formed a spiral.

It was perfectly evident from these experiments, that as many polar arrangements may be formed as chords can be drawn in circles surrounding the wire; and so far these phenomena agree with your idea of revolving magnetism; but I shall quit this subject, which I hope you will yourself elucidate for the information of the Society, to mention some other circumstances and facts belonging to the enquiry.

Supposing powerful electricity to be passed through two, three, four, or more wires, forming part of the same circuit parallel to each other in the same plane, or in different planes, it could hardly be doubted that each wire, and the space around it, would become magnetic in the same manner as a single wire, though in a less degree; and this I found was actually the case. When four wires of fine platinum were made to complete a powerful voltaic circuit, each wire exhibited its magnetism in the same manner, and steel filings on the sides of the wires opposite attracted each other.

As the filings on the opposite sides of the wire attracted each other in consequence of their being in opposite magnetic states, it was evident, that if the similar sides could be brought in contact, steel filings upon them would repel each other.—This was very easily tried with two voltaic batteries arranged parallel to each other, so that the positive end of one was opposite to the negative end of the other: steel filings upon two wires of platinum joining the extremities strongly repelled each other. When the batteries were arranged in the *same* order, *i. e.* positive opposite to positive, they attracted each other; and wires of platinum (without filings) and fine steel wire (still more strongly) exhibited similar phenomena of attraction and repulsion under the same circumstances.

As bodies magnetized by electricity put a needle in motion, it was natural to infer that a magnet would put bodies magnetized by electricity in motion; and this I found was the case. Some pieces of wire of platinum, silver, and copper, were placed separately upon two knife edges of platinum connected with two ends of a powerful voltaic battery, and a magnet presented to them; they were all made to roll along the knife edges, being attracted when the north pole of the magnet was presented, the positive side of the battery being on the right hand, and repelled when it was on the left hand; and vice versa, changing the pole of the magnet. Some folds of gold leaf were placed across the same apparatus, and the north pole of a powerful magnet held opposite to them; the folds approached the magnet, but did not adhere to it. On the south pole being presented, they receded from it.

I will not indulge myself by entering far into the theoretical part of this subject; but a number of curious speculations cannot fail to present themselves to every philosophical mind, in consequence of the facts developed; such as whether the magnetism of the earth may not be owing to its electricity, and the variation of the needle to the alterations in the electrical currents of the earth in consequence of its motions, internal chemical changes, or its relations to solar heat; and whether the luminous effects of the auroras at the poles are not shown, by these new facts, to depend on electricity. This is evident, that if strong electrical currents be supposed to follow the apparent course of the sun, the magnetism of the earth ought to be such as it is found to be.

But I will quit conjectures, to point out a simple mode of making powerful magnets, namely, by fixing bars of steel

across, or circular pieces of steel fitted for making horse-shoe magnets, round the electrical conductors of buildings in elevated and exposed situations.*

The experiments detailed in these pages were made with the apparatus belonging to the Royal and London Institution; and I was assisted in many of them by Mr. PEPYS, Mr. ALLEN, and Mr. STODART, and in all of them by Mr. FARADAY.†

I am, my dear Sir,

very sincerely yours,

HUMPHRY DAVY.

Lower Grosvenor Street,
Nov. 12, 1820.

* There are many facts recorded in the Philosophical Transactions which prove the magnetizing powers of lightning; one in particular, where a stroke of lightning passing through a box of knives, rendered most of them powerful magnets. See Philosophical Transactions, No. 157, p. 520; and No. 437, p. 57.

† All the experiments detailed in this paper, except those mentioned p. 15, were made in the course of October, 1820; the last arose in consequence of a conversation with Dr. WOLLASTON, and were made in the beginning of November. I find, by the Annales de Chimie et de Physique, for September, which arrived in London November 24, that M. ARAGO has anticipated me in the discovery of the attractive and magnetizing powers of the wires in the voltaic circuit; but the phenomena presented by the action of common electricity (which I believe as yet have been observed by no other person), induce me still to submit my paper to the Council of the Royal Society. Before any notice arrived of the researches of the French philosophers, I had tried, with Messrs. ALLEN and PEPYS, an experiment, which M. ARAGO likewise thought of,—whether the arc of flame of the voltaic battery would be affected by the magnet; but from the imperfection of our apparatus, the results were not decisive. I hope soon to be able to repeat it under new circumstances.

I have made various experiments, with the hope of affecting electrified wires by the magnetism of the earth, and of producing chemical changes by magnetism; but without any successful results.

Since I have perused M. AMPÈRE's elaborate treatise on the electro-magnetic phenomena, I have passed the electrical shock along a spiral wire twisted round a glass

tube containing a bar of steel, and I found that the bar was rendered powerfully magnetic by the process.

Without meaning to offer any decided opinion on that Gentleman's ingenious views, I shall beg permission to mention two circumstances, which seem to me unfavourable to the idea of the identity of electricity and magnetism; 1st, the great distance to which magnetism is communicated by common electricity (I found that a steel bar was made magnetic at 14 inches distance from a wire transmitting an electric shock from about 70 feet of charged surface); and, 2d. that the effect of magnetizing at a distance by electricity takes place with the same readiness through air and water, glass, mica, or metals; *i. e.* through conductors and non-conductors.

III. *A Communication of a singular fact in Natural History. By the Right Honourable the Earl of* MORTON, *F. R. S. in a Letter addressed to the President.*

Read November 23, 1820.

MY DEAR SIR,

I YESTERDAY had an opportunity of observing a singular fact in Natural History, which you may perhaps deem not unworthy of being communicated to the Royal Society,

Some years ago, I was desirous of trying the experiment of domesticating the Quagga, and endeavoured to procure some individuals of that species. I obtained a male; but being disappointed of a female, I tried to breed from the male quagga and a young chesnut mare of seven-eighths Arabian blood, and which had never been bred from: the result was the production of a female hybrid, now five years old, and bearing, both in her form and in her colour, very decided indications of her mixed origin. I subsequently parted with the seven-eighths Arabian mare to Sir GORE OUSELEY, who has bred from her by a very fine black Arabian horse. I yesterday morning examined the produce, namely, a two-years old filly, and a year-old colt. They have the character of the Arabian breed as decidedly as can be expected, where fifteen-sixteenths of the blood are Arabian; and they are fine specimens of that breed; but both in their colour, and in the hair of their manes, they have a striking resemblance to the quagga. Their colour is bay, marked more or less ke the quagga in a darker tint. Both

are distinguished by the dark line along the ridge of the back, the dark stripes across the fore-hand, and the dark bars across the back part of the legs. The stripes across the fore-hand of the colt are confined to the withers, and to the part of the neck next to them; those on the filly cover nearly the whole of the neck and the back, as far as the flanks. The colour of her coat on the neck adjoining to the mane is pale, and approaching to dun, rendering the stripes there more conspicuous than those on the colt. The same pale tint appears in a less degree on the rump; and in this circumstance of the dun tint also she resembles the quagga.

The colt and filly were taken up from grass for my inspection, and, owing to the present state of their coats, I could not ascertain whether they bear any indications of the spots on the rump, the dark pasterns, or the narrow stripes on the forehead, with which the quagga is marked. They have no appearance of the dark line along the belly, or of the white tufts on the sides of the mane. Both their manes are black; that of the filly is short, stiff, and stands upright, and Sir Gore Ouseley's stud groom alleged that it never was otherwise. That of the colt is long, but so stiff as to arch upwards, and to hang clear of the sides of the neck; in which circumstance it resembles that of the hybrid. This is the more remarkable, as the manes of the Arabian breed hang lank, and closer to the neck than those of most others. The bars across the legs, both of the hybrid and of the colt and filly, are more strongly defined, and darker than those on the legs of the quagga, which are very slightly marked; and though the hybrid has several quagga marks, which the colt and filly have not, yet the most striking, namely, the

stripes on the fore-hand, are fewer and less apparent than those on the colt and filly. These circumstances may appear singular; but I think you will agree with me, that they are trifles compared with the extraordinary fact of so many striking features, which do not belong to the dam, being in two successive instances, communicated through her to the progeny, not only of another sire, who also has them not, but of a sire belonging probably to another species; for such we have very strong reason for supposing the quagga to be.

I am, my dear Sir,

Your faithful humble servant,

Dr. W. H. Wollaston. MORTON.

P. S. I have requested Sir GORE OUSELEY to send me some specimens of hair from the manes of the sire, dam, colt, and filly; and I shall write to Scotland for specimens from those of the quagga and of the hybrid.

I am not apt to build hypotheses in a hurry, and have no predilection either for or against the old doctrine of impressions produced by the imagination; but I can hardly suppose that the imagination could pass by the white tufts on the quagga's mane, and attach itself to the coarseness of its hair.

Wimpole Street, August 12th, 1820

Note by Dr. Wollaston.

By the kindness of Sir GORE OUSELEY, I had an opportunity of seeing the mare, the Arabian horse, the filly, and the colt, and of witnessing how correctly they agreed with the description given of them by Lord MORTON.

Having shortly afterwards described the circumstances to my friend Mr. GILES, I found that he had observed some facts of nearly equal interest, of which, at my request, he has since sent me the following account.

IV. *Particulars of a fact, nearly similar to that related by Lord* MORTON, *communicated to the President, in a letter from* DANIEL GILES, *Esq.*

Read November 23, 1820.

IN answer to your enquiries, I will now give the best account I can of my sow and her produce.

She was one of a well known black and white breed of Mr. WESTERN, the Member for Essex. About ten years since I put her to a boar of the wild breed, and of a deep chesnut colour, which I had just received from Hatfield House, and which was soon afterwards drowned by accident. The pigs produced (which were her first litter) partook in appearance of both boar and sow, but in some the chesnut colour of the boar strongly prevailed.

The sow was afterwards put to a boar of Mr. WESTERN's breed (the wild boar having been long dead). The produce was a litter of pigs, some of which we observed, with much surprize, to be stained and clearly marked with the chesnut colour which had prevailed in the former litter.

This sow had afterwards another litter of pigs by a boar of Mr. WESTERN's breed, and I think, and so does my bailiff, that some of these were also slightly marked with the chesnut colour; but though we noticed the recurrence with surprize, it is so long since, that our recollection is much less perfect than I wish it to be.

I should observe, that I have known Mr. WESTERN's breed many years, but never in any other instance observed the least appearance of the chesnut colour.

Believe me, &c.

DANIEL GILES.

Youngsbury, Nov. 10, 1820.

V. *The Croonian Lecture. Microscopical observations on the following subjects. On the Brain and Nerves; showing that the materials of which they are composed exist in the blood. On the discovery of valves in the branches of the vas breve, lying between the villous and muscular coats of the stomach. On the structure of the Spleen.* By Sir EVERARD HOME, *Bart. V. P. R. S.*

Read December 7th, 1820.

THE Croonian Lectures for the three preceding years, contain Mr. BAUER's microscopical observations on the blood. That fluid we find is made up of a greater number of ingredients than it was known to contain; indeed we find in it materials ready prepared, for the formation of most of the structures of an animal body.

In the present Lecture, the brain and nerves form the first subject of investigation. Having found upon a former occasion that the retina is perfectly transparent in the living body, and is only rendered visible by coagulation after death, this, the only expansion of medullary substance in the body with which I am acquainted, was examined by Mr. BAUER in the microscope.

He found the optic nerve to consist of many bundles of extremely delicate fibres, formed of minute globules connected together by a gelatinous substance, which readily dissolves in water. The dimensions of the globules, measured on the micrometer, explained in the preceding Lectures, are from $\frac{1}{2000}$ to $\frac{1}{4000}$ parts of an inch, mixed with very few

of $\frac{1}{3000}$ parts, the size of the red globules deprived of their colouring matter.

The retina appeared as a continuation of the bundles composing the optic nerve, and consists entirely of the same sized globules connected into fibres, and forming bundles, which go off distinctly from the end of the nerve, like rays: towards the circumference they almost disappear, and end in smooth membrane.

The whole retina is interwoven with innumerable blood vessels, both arteries and veins; the gelatinous substance that holds the globules together, dissolves in water very readily; so that if the parts are soaked in water for three or four days with a portion of the optic nerve, they become a mass of globules, and the blood vessels, when separated, form a beautifully delicate net-work, their branches anastomosing freely with one another. These appearances are represented in Pl. II. fig. 4. magnified 400 times.

By the discovery of this transparent substance, we become acquainted with the nature of the medullary structure of the nerves; and can form some idea of their action, which, till now, I confess myself to have been totally unacquainted with. The nerves, as well as the retina, are composed of this newly discovered transparent substance, which is very elastic, and soluble in water, and globules of $\frac{1}{2000}$ and $\frac{1}{4000}$ parts of an inch in diameter. Its transparency and solubility account for its having remained concealed; and were it not coagulable, in which state it becomes opaque, its existence might even now be considered as equivocal.

Before I say more of this transparent jelly, I will state Mr. BAUER's observations on the structure of the brain, of which

it makes an essential part. If the mass of the brain is kept in water for 48 hours, and a thin slice is cut from the medullary part of the cerebrum, and laid upon a glass plate previously wetted with water, and a drop of water is allowed to fall upon the slice, holding the glass a little obliquely, so that the water must run across the surface of the glass, the force with which it moves is sufficient to break down the medullary substance of the brain, so as to bring distinctly into view innumerable loose globules, many fragments of fibres of single rows of globules, and bundles of fibres, some of them of considerable length, as represented in Pl. II. fig. 1.

If the substance of the brain is laid upon a piece of dry glass, and the separation of its parts is attempted by instruments, it is impossible to effect it, as the viscous mucus adheres strongly to the glass, and the substance would be indistinctly daubed on the glass, in the manner of a pigment, a state in which the globules are not discernible.

It is impossible to distinguish the fibres composed of globules, in an opaque state of the substance; for, although in the section of any part of the brain, by means of a very strong magnifying lens, lines are discernible, these lines are produced by the light and shade on the substance, and only denote the bundles of fibres of which the brain is composed, but not the simple globular fibres.

The gelatinous mucus seems to dissolve readily, and mix with the water; and, being perfectly colourless and transparent, is entirely invisible while the substance of the brain is fresh, or whilst it is immersed in water; but if the water is left to evaporate, and the substance gets dry, the mucus collects round the loose globules and fibres in considerable

quantity, or forms irregular flakes or splotches upon the glass, perfectly transparent, and of a yellowish tinge, as represented in Pl. II. fig. 2.

If a portion is cut off from the brain in a fresh state, before it has been put in water, and laid upon a dry glass plate, and covered by a cup, so as to prevent evaporation, a perfectly colourless aqueous fluid is exuded, which evaporates on exposure to the air, and hardly leaves any mark upon the glass.

The cortical substance of the cerebrum contains also a fluid resembling the serum of the blood; it has a yellower tint than the fluids in the medullary substance, or any other part of the brain; and, when dry, it assumes the glassy appearance, and forms the same cracks that the serum does when dried on glass.

The above are all the visible materials that can be distinguished in the different parts of the human brain by means of the microscope; and, making allowance for slight modifications, are the same in different parts of the organ.

The globules are from $\frac{1}{3400}$ to $\frac{1}{4000}$ of an inch in diameter; but the general or predominant size is $\frac{1}{3200}$. They are semi-transparent, and of a white colour, arranged into fibres of single globules, and seem to be held together by the viscid mucus. The fibres form bundles connected in the same way.

The principal difference in the appearance of the different parts of the brain, consists in the proportions the quantity of mucus and fluids bear to the quantity of globular tissue in the same part, and in some respects in the size of the globules; as for instance, the cortical substance of the cerebrum and cerebellum, (which are in all respects alike) consists

chiefly of globules from $\frac{1}{3300}$, to $\frac{1}{4000}$ of an inch in diameter; and the smaller globules prevail. The single globular fibres are not so readily distinguished as in the other parts of the brain; the gelatinous mucus and fluid resembling serum, are very abundant. The finest and most delicate branches of the arteries and veins are only found in the cortical substance.

The medullary substance of the cerebrum and cerebellum differs from the above in the large globules prevailing; the mucus being more tenacious and less in quantity in proportion to the globular tissue, and the single globular fibres being more distinct, and the arterial and venal branches being larger.

The crura cerebri and cerebelli resemble in general the medullary substance, only that the mucus and fluids are more abundant; and there appears a greater proportion of mucus than globules; the blood vessels are larger than in the medullary substance.

The medulla oblongata, the corpora pyramidalia and olivaria have nearly the same structure as the medullary substance; the single globular fibres, and their bundles, are composed of the larger globules; the mucus, however, is very abundant, and is sooner dissolved in water than the mucus in any other part of the brain.

The pons verolii is principally composed of globules $\frac{1}{3300}$ of an inch; the fibres not quite so distinct as in the medulla oblongata; the mucus very abundant. The medulla spinalis has the globules of $\frac{1}{2400}$ to $\frac{1}{3300}$ of an inch predominant; the mucus and fluid less tenacious, but in greater quantity than in any part of the brain; for this reason, the single globular fibres are not so readily discovered; for if

the part is not sufficiently soaked in water, they cannot be separated; and if macerated too much, the whole is dissolved into a mass, like cream. The corpus callosum resembles the medulla spinalis, but contains a greater quantity of globules $\frac{1}{2400}$ of an inch than any other part of the brain; the quantity of mucus and fluid are at least equal to the globular tissue.

Every part of the substance of the brain is pervaded by innumerable blood vessels, which are of considerable size towards the centre, but branch out to an extreme degree of minuteness, less than the half diameter of a red globule with its colouring matter; and even when of that size the fluid they carry is red, as in Pl. II. fig. 4.

These arteries in the brain never anastomose, as in the retina; their branches are accompanied by veins of still less diameter, having valves. The valves are at very short distances, particularly near their extremities; and when the brain is fresh, these veins contain a red fluid. See Fig. 8.

The circumstances noticed by Mr. BAUER, namely, the cortical substance of the cerebrum and cerebellum being made up of the small globules; containing the gelatinous fluid soluble in water in great abundance; and having branches more minute than the other arteries of the brain; also the corresponding veins having valves similar to those found in absorbent ves sel and their canals carrying a red fluid,—throw considerable light upon the functions of the brain, and show that the cortical substance is one of the most essential parts of this organ, although the pons varolii, as the commune vinculum between the different portions of this complicated structure, may be the most essential to life.

That the cortical part of the brain is the seat of memory, is an opinion I have long entertained, from finding that any continued undue pressure upon the upper anterior part of the brain entirely destroys memory, and a less degree materially diminishes it. Pressure upon the dura mater, where the skull has been trepanned, puts a temporary stop to all sense, which is restored the moment that pressure is removed; and the organ appears to receive no injury from repeated experiments of this kind having been made. In hydrocephalus, when the fluid is in large quantity, and there only remains the cortical part of the brain and pons verolii connecting it to the cerebellum, all the functions go on, and the memory can retain passages of poetry, so as to say them by heart; but a violent shake of the head produces instant insensibility. Pressure in a slight degree upon the sinciput, produced in one case complete derangement, with violent excesses of the passion of lust, both of which went off upon removing, by the crown of the trepan, the depressed bone.

The veins being so minute, and being supplied with valves, explains the circumstance of lymphatics never having been met with in this organ; these veins performing that office, carrying the absorbed matter into the superior longitudinal sinus, which appears more a reservoir than a vein; for the fluid that passes through it is not simply circulating blood; it contains the colouring matter in a decomposed state as black as ink, a change we shall find it undergoes in the spleen after death.

The superior longitudinal sinus may be considered as the common receptacle of the absorbent veins of the pia mater

from its triangular form, it always remains full. The aqueous liquid, by which the ventricles are filled, varying in quantity, answers the purpose of equalizing internal pressure.

As the transparent mucus not only is one of the most abundant materials of which the brain itself is composed, but is the medium by which the globules of the retina are kept together, and serves the same purpose in the medullary texture of the nerves, there can be no doubt that the communication of sensation and volition, more or less, depend upon it. And it would appear from the following case, that when parts are regenerated, they contain a sufficient quantity of this mucus to connect them with the nerves of the body, and enable them to partake of its sensibility.

A lady, who had a wound on the breast in a healing state, had a prominent spot of a black colour suddenly make its appearance on the surface; it was very tender to the touch; next day it disappeared, and the tenderness was gone. This must have been blood coagulated upon the termination of a nerve, and therefore the impression made by touching it was communicated along the nerve; but when it was absorbed, the bare nerve received a coating of coagulable lymph, and there was no more pain.

Mr. HUNTER's comprehensive mind grasped at the idea of the existence of something of this kind, although he had not arrived at a knowledge of the substance employed to produce the effect. He said, that so wonderful was the connection between the brain and every structure of the body, that it was to be explained in no other way than by considering, that the materia vitæ was every where; that it was in two forms collected into one mass in the brain, which he called coacervata;

and diffused through the body, which he termed diffusa, and the nerves communicated between them. This grand idea of Mr. Hunter's, Mr. Bauer, by his discovery of this transparent mucus, soluble in water, has realized.

To complete the investigation of this subject, it only remained to determine, whether this transparent substance, soluble in water, is actually an ingredient in the blood, or is formed after the first changes of that fluid into the solids of the body have taken place.

To ascertain this point, two ounces of blood were drawn from the arm of a healthy man, and allowed to stand at rest till all the serum separated from the coagulum, which required 36 hours; the serum was then carefully poured off, and the phial filled up with distilled water, and the changes that occurred were attended to. In 24 hours, the upper parts of the coagulum, particularly at the edges, became tumid, apparently from having imbibed some of the water. This part was of a light red colour, and semi-transparent. A small portion of it was cut off and put into a saucer with distilled water, covered over by a watch glass. In 24 hours, carbonic acid gas was seen in bubbles round the edge of the watch-glass, the colouring matter had mixed with the water, and the whole of the substance was nearly dissolved.

From this experiment, confirmed by many others, this mucus is not only readily discovered in the blood, but proves to be the medium by which the colouring matter is attached to the surface of the red globules; and therefore when these red globules are put into water, they lose their colour from the medium dissolving by which it was attached to them.

From this investigation of the blood, it appears that the principal materials of which the body is composed, are met with in the blood. The fat is by many considered a secretion; for this opinion there is however no foundation. That the fat is formed in the colon, and is thence taken up into the blood vessels, and distributed to the different parts of the body, is sufficiently proved by the mode in which adipocere is made, and by the observations on the colon in different animals, that have been long since laid before the Society. No direct experiment has been made, that I am acquainted with, for detecting the presence of fat in the blood; possibly the reason is, that its failure would prove nothing, since the blood contains an alkali with which the oil will become united. That I might not be said to have neglected this part of the enquiry, I instituted the following experiment.

Twelve ounces of blood were drawn from the arm into a glass vessel of a globular form capable of containing a pint, with a tube rising out of the globe six inches long, and half an inch in diameter; at the end of 24 hours the serum was poured off, and the vessel and tube filled up to the orifice with distilled water; after this vessel had remained 24 hours at rest, no appearance of oil took place upon the surface. The coagulum was then broken down by a long wire, which produced an immediate evolvement of carbonic acid gas, in such quantity that the water fell in the tube an inch in length; but there was no appearance of oil seen on the surface. The blood was examined for several days successively; it became very offensive, but showed no appearance of oil. In the blood of the salmon and skate, oil is met with in such quantity as to render blotting paper greasy.

In the skate, the blood globules are of a very large size, and have an oval form; the colouring matter in them is of a light yellow; they very readily change their appearance when decomposition begins to take place: at this time the oval becomes flattened, and the central part appears more dense than the margin, by which it is surrounded in the form of a ring, and when this ring dissolves, the globule it contained is seen of a spherical form, and the surrounding fluid has oil floating in it, distinguishable by the naked eye, as well as by other tests.

The globules are represented in Pl. III. fig. 5. magnified 4000 diameters.

The salts in the skate's blood must be very abundant, since they are found in it crystallized, as represented in Pl. III. fig. 6: magnified 200 times.

On the branch of the vas breve carrying the fluids from the stomach through the splenic vein to the vena portarum.

The discovery of valvular vessels in the brain, acting as absorbents in that organ, immediately led me to suspect that there must be a similar provision for carrying off the fluids taken into the stomach, whenever the quantity or quality interfered with the process of digestion. To do this by the route of the thoracic duct, was not only too circuitous to correspond with the general simplicity of the operations of nature, but was mixing these heterogeneous liquids in too crude a state, with the general circulation of the blood. That there was some unusual mode of conveying fluids from the stomach to the

urinary bladder, I have upon a former occasion established, since they arrived there when both the pylorus and thoracic duct were tied up, and the spleen was removed out of the body; but till the fact of valvular vessels supplying the office of absorbents was ascertained, any opinion respecting the route of fluids from the stomach, must continue to be entirely hypothetical.

Upon the present occasion, through Mr. BAUER's means, I am not only enabled to demonstrate vessels so constructed in the coats of the stomach, but to give abundant collateral evidence of their acting as absorbents, even more than can be produced respecting those of the brain.

It immediately suggested itself to me, that this was the probable use of the branches of the vas breve, the presence of which upon the coats of the stomach, so well supplied with veins from other trunks, is not easily accounted for.

In the first instance, with the assistance of Mr. CLIFT, I injected the splenic artery, and requested Mr. BAUER to ascertain, whether any minute branches, spread upon the great curvature of the stomach, in a contrary direction to those injected arteries, had valves. Such vessels were found, and quite empty. They had valves very distinctly marked: he showed them to me, so as perfectly to satisfy me of the fact. Having got thus far, I requested the assistance of Mr. CHEVALIER, House Surgeon to St. George's Hospital, who has given, at different times, considerable time and attention to preparing the stomach and spleen for Mr. BAUER's observations; which he has been better enabled to do, from being more in the habit of injecting the blood vessels, than students in surgery

generally are; Mr. CLIFT's important occupations at the College depriving me of his valuable assistance. I requested Mr. CHEVALIER to inject, as minutely as possible, the branches of the splenic artery and vein going to the stomach. In one instance, he succeeded so well that the arteries were filled to the most minute branches, and some of the injection had passed into the stomach, without any apparent rupture of the vessel. No part of this coloured injection had got into the veins, which in other parts of the circulation generally happens. Between the villi and the muscular coats of the stomach there is a very fine elastic cellular membrane: it admits of being drawn out to more than three times its natural thickness; and it was by doing so, Mr. BAUER caused these smaller arteries to be exposed, and, along with them, small valvular vessels quite empty; the valves were very numerous, and nearly at equal distances. In tracing these towards the cavity, they became indistinct just as they entered the villi. These appearances are shown in Pl. III. fig. 2, 3, 4. This representation of the valves in these vessels, as well as that of the valves of the vessels in the brain, may be considered as demonstrations of the fact; and still more valuable than preparations, since the appearance can be better preserved.

To show the course of the absorbed fluids, as well as to give a clear idea of every thing connected with so *important* a discovery, a drawing of the spleen, the vas breve, and cardiac portion of the stomach, is annexed [Pl. IV;] and as the trunk of the splenic vein forms one of the trunks of the vena portæ, the liquids are directly carried to the liver,

forming a part of the materials employed in producing the bile; the remainder only returning by the vena cava to the heart.

This additional quantity of liquids passing along the splenic vein, accounts for its being five times the size of the artery, as well as for the blood in that vein having a greater proportion of serum than the blood in any other, which has been long asserted, and which I found by actual experiment to be the case; but being unable to account for it, as I can now, I was willing to admit that the mode of measuring might be erroneous.

On the structure and uses of the spleen.

In the different investigations that have been made of this organ, the following facts have been ascertained; but *still* neither the more minute texture, nor the ultimate use has been, till now, discovered.

It was known that a man could live without his spleen; but there is no satisfactory account upon record of the inconvenience he suffered from its loss.

It has been ascertained that the spleen, under different circumstances, is larger or smaller in size. In an ass after fasting two days, it was half the size that it was met with in another, killed two hours after drinking freely. In the diminished state there are no corpuscules; in the enlarged state they are very numerous.

The spleen was believed to consist of a net-work of ligamentous structure, with numerous arterial and venal branches, having cells containing small corpuscules or glands; but this

appearance vanishes when the parts are more minutely examined. Its structure was made out in the following manner.

Wednesday, August 23, 1820, at 12 o'clock, a healthy large spleen, taken from a man twenty-eight years of age, was cut into eight transverse slices, nearly of the same thickness; four of these were put into one flat dish, and four into another; both of these were filled with distilled water; no colouring matter was given out, although the surfaces had all a red colour; the cells were unusually distinct, and had a degree of uniformity in their appearance. On examining the cells under water with a common lens, they appeared full; on turning them over in the water, something colourless fell out, carrying no colouring matter along with it. The same thing happened on turning up the opposite side. This appearance is represented in Pl. VI. fig. 1. and 2.

Aug. 24th, 12 o'clock. Some colouring matter was discharged round each slice, forming a circle round it, but not in contact with the edge. It had the appearance of red serum, but not that of the globules parting with their colour; the surface lost its red colour, becoming darker. The cells were examined by a lens, when those on the upper surface appeared hollow and empty, but those on the under surface appeared full of a mucus soaked in water, and a film was spread over the surface of several of them. The water was changed every day.

25th, at 12 o'clock, both the upper and under surfaces were obscured by flocculent mucus over the cells, which were equally distinct; the red colour was discharged in greater quantity.

26th. Both surfaces had the mucus filling the cells, of a

paler red colour than the surrounding substance, looking exactly like plum-pudding stone. The cells were tumid, rising above the surface.

27th. No material change.

28th. The surface of the cells had become flat, the discharge of colouring matter considerable.

29th. The cells still flatter, and a light coloured point in the centre of each.

30th. No change, the colouring matter still discharging.

31st. The surface so slimy that the slices were very slippery; no other change, except a number of round deep black spots: in some places they appeared as if filling the orifices of divided arteries; in others, as if the surface of one or two cells was blackened

September 1st. The black spots more numerous; on the surface of the water much colouring matter, but no mucus separated.

2nd. A greater extent of black surface; more mucus.

4th. The colouring matter nearly gone; more black along the surface; mucus on both surfaces; their cells more distinct.

5th. Black colour more extended; little mucus or colouring matter.

6th. The slice become very putrid; cells as distinct as at first; more extension of black; no colouring matter; little mucus.

8th. Upper surface all black; no appearance of cells, although seen on the under surface. The black colour was produced by the colouring matter becoming putrid.

12th. The whole substance one mass of branches of vessels; every thing else dissolved.

August 29th. in the same year. The spleen of a woman (who had taken little food for some time,) hardly more than one third the size of the other, contained no cells, and consequently no corpuscules; was treated in the same manner, and the changes it underwent were the same.

These different appearances are represented in Plates VI. VII. VIII. each having a separate explanation.

The spleen, from this account, consists of blood vessels, between which there is no cellular membrane, and the interstices are filled with serum, and the colouring matter of the blood from the lateral orifices in the veins, when these vessels are in a distended state; which serum is afterwards removed by the numberless absorbents belonging to the organ, and carried into the thoracic duct by a very large absorbent trunk.

That all the apparent fibres are vascular, is proved by the representation in Pl. III. fig. 1, in which they are minutely injected; and the injection is carried into the cells, and moulded into their form. The lymph globules carry along with them into the interstices carbonic acid gas, and the mucus soluble in water, in great abundance; but no blood globules, since none are found in the cells. As soon as the lymph is at rest, the carbonic acid gas being let loose, forms the cells that surround the lymph globules, the sides of which are held together by the mucus, putting on the appearance of corpuscules without colour, and are thus mistaken for glands; the gas is absorbed by the blood in the arteries and veins.

The spleen, from this mechanism, appears to be a reservoir for the superabundant serum, lymph globules, soluble

mucus, and colouring matter, carried into the circulation immediately after the process of digestion is completed.

EXPLANATION OF THE PLATES.

PLATE II.

IN this Plate several small parts of glass micrometers are represented, in which the inch is divided into 400 parts in diameter, which divides the superficies into 160,000 parts; so that every object is magnified 400 times in diameter, and 160,000 in superficies.

Fig. 1. In the first square at A, are represented the globules of the cerebrum that are predominant. These, it is evident, are $\frac{1}{3200}$ part of a lineal inch. The rest of the micrometers of fig. 1. contain many loose globules of various sizes, and fragments of bundles and simple globular fibres of the medullary substance of the cerebrum in a fresh state, immersed in water.

Fig. 2. represents the same objects in a dried state, when the accumulated mucus, and some newly produced globules, become visible.

Fig. 3. represents a very small portion of the medullary substance of the cerebrum diluted with water; also displaying fragments of single globular fibres, many loose globules, and a portion of the venal branches with valves.

Fig. 4. represents a small portion of the retina of the human eye diluted with water, consisting of loose globules and globular fibres of the same size as those of the brain in its various

parts; also a branch of an artery whose anastomoses compose a beautiful net-work almost over the whole membrane of the retina.

This representation of an arterial branch, may serve as an illustration of those that pervade every part of the substance of the human brain; but such branches do not any where anastomose except in the retina.

Plate III.

This Plate consists of six figures. The first representing a portion of the spleen. The second, third, and fourth, the vessels with valves passing from the internal membranes of the stomach. The fifth and sixth, the blood globules and salts in the blood of the skate.

Fig. 1. A surface of $\frac{5}{16}$ parts of a square inch of a slice of the spleen of a child five years old, minutely injected for the arteries; magnified 8 diameters. It shows the termination of the arteries in the empty cells, which are also filled with the injection, in the same manner as the corpuscules themselves are originally formed.

Fig. 2. A small slice of the coats of the cardiac portion of the human stomach, magnified 8 diameters, from a man 48 years old. The blood vessels were minutely injected soon after death, the arteries with red, the veins with yellow; the cellular membrane, or rather the filamentous substance between the villous and muscular coat, is in this figure stretched out to more than three times its natural thickness, and it is in this space that the empty valvular vessels are distinctly shown.

Fig. 3. and 4. Portions of the small valvular vessels, showing that they vary in appearance; magnified 400 diameters.

Fig. 5. Some globules of the blood of the skate floating in the coloured serum. The blood was taken from the heart of a fish quite alive at the time the heart was opened. In these globules the enveloping substance is quite smooth, and the globules are perfectly in the shape of eggs; and the contained spherical globules are not visible.

B. A group of seven globules floating in the serum, diluted in water, upon a piece of glass; the globules are attracted and adhere closely to each other, and become flat; the inner spherical globule is distinctly seen, the enveloping matter forming an elevated rim round it.

C. represents the same group, after having been 20 minutes in the diluted serum; the enveloping substance is dissolved; and after all the moisture is evaporated, the spherical globules appear still closer drawn together, and quite clear and distinct, and the enveloping substance almost entirely gone, leaving only some greasy marks on the glass; magnified 400 diameters.

Fig. 6. A group of crystals of salts formed on the surface of the decomposed blood of the skate, when the blood begins to putrify; magnified 200 diameters.

PLATE IV.

The human stomach and spleen in their relative situation, of the natural size, from a young man of 15 years of age.

A. shows the vas breve where it joins the splenic vein. When the branches are traced to the stomach, some dip in between its coats, the others run on the surface, anastomosing with the branches of the other trunks belonging to that viscus.

PLATE V.

The human spleen of the natural size, from a man 48 years of age. The arteries and veins are injected with the same colour, showing that they run in pairs, enclosed in a common theca.

PLATE VI.

Represents two sections of the spleen, one prior to maceration, the second after being in distilled water three days.

Fig. 1. A transverse section of the same spleen, as represented in Pl. V. of the natural size. Upon immersing it in distilled water, the cells emptied themselves of lymph globules; and upon turning it over, the same thing happened to the opposite side.

Fig. 2. A surface of $\frac{2}{16}$ parts of a square inch of Fig. 1. showing distinctly the cells; and also a small portion of a vein laid open, exposing the perforations met with near the sinus in the concave part of the spleen; magnified 8 diameters.

Fig. 3. A transverse slice of the spleen of a boy 15 years of age. This portion had been kept in distilled water, in an open dish, for three days, when the mucus, which with the globules forms the lining of the cells of the spleen, was so much swoln, that not only all the cells were filled with it, but the mucus was raised above the surface, giving it an uneven appearance. The parts are of the natural size.

Fig. 4. A surface of $\frac{2}{16}$ parts of a square inch of the above slice, magnified 8 diameters.

PLATE VII.

Two sections of the spleen in different stages of maceration.

Fig. 1. A transverse section of the same spleen, as represented in Pl. VI. fig. 1. After remaining 12 days in distilled water in an open dish the mucus is nearly dissolved, and the black spots

(which are produced by the colouring matter becoming putrid) are rapidly extending; the cells are nearly empty. Natural size.

Fig. 2. A surface $\frac{3}{16}$ parts of a square inch of the above slice, magnified 8 diameters.

Fig. 3. A transverse slice of the same spleen soaked in distilled water 90 days, when the mucus was almost wholly dissolved, and all the colouring matter discharged; but this slice having been kept in a deep glass quite full of distilled water, and closed with a glass stopple, no atmospheric air could penetrate; the mucus and colouring matter were washed out before putrefaction could take place; the remaining globular substance then was so loose, that by the least motion of the section of the spleen the margins of the cells crumbled and fell to pieces. Of the natural size.

Fig 4. A surface of $\frac{3}{16}$ parts of a square inch of the above slice, magnified 8 diameters; showing more distinctly the crumbling of the sides of the cells.

PLATE VIII

Represents a section of the spleen completely macerated.

Fig. 1. A transverse slice of the spleen of a boy ten years of age; the spleen being kept in water 48 days, the mucus and the colouring matter were entirely dissolved and discharged; and fresh water being pumped every second day upon it, the globular substance was gradually completely washed out, and the arteries, to their most minute branches, became perfectly clear and distinct, no other parts being left.

Fig. 2. A surface of $\frac{3}{16}$ parts of a square inch of the above figure, magnified 8 diameters.

A

Fig. 1.

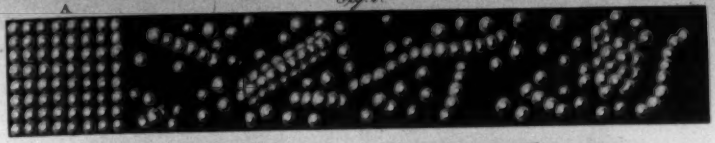
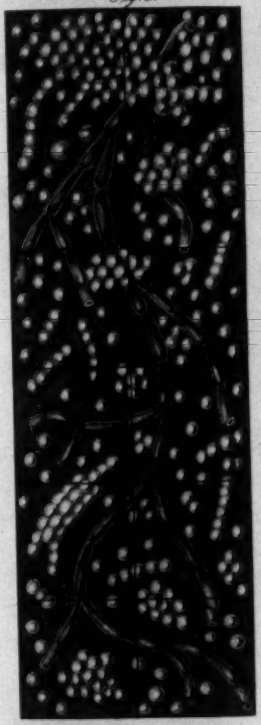


Fig. 3.



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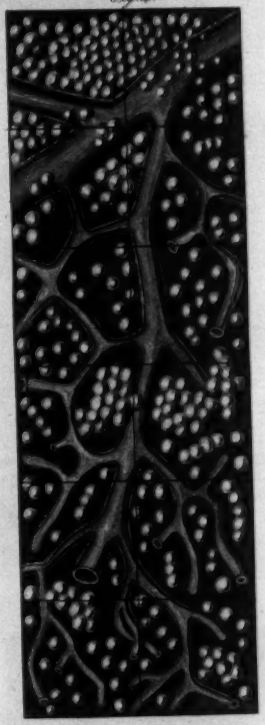

Fig. 2.

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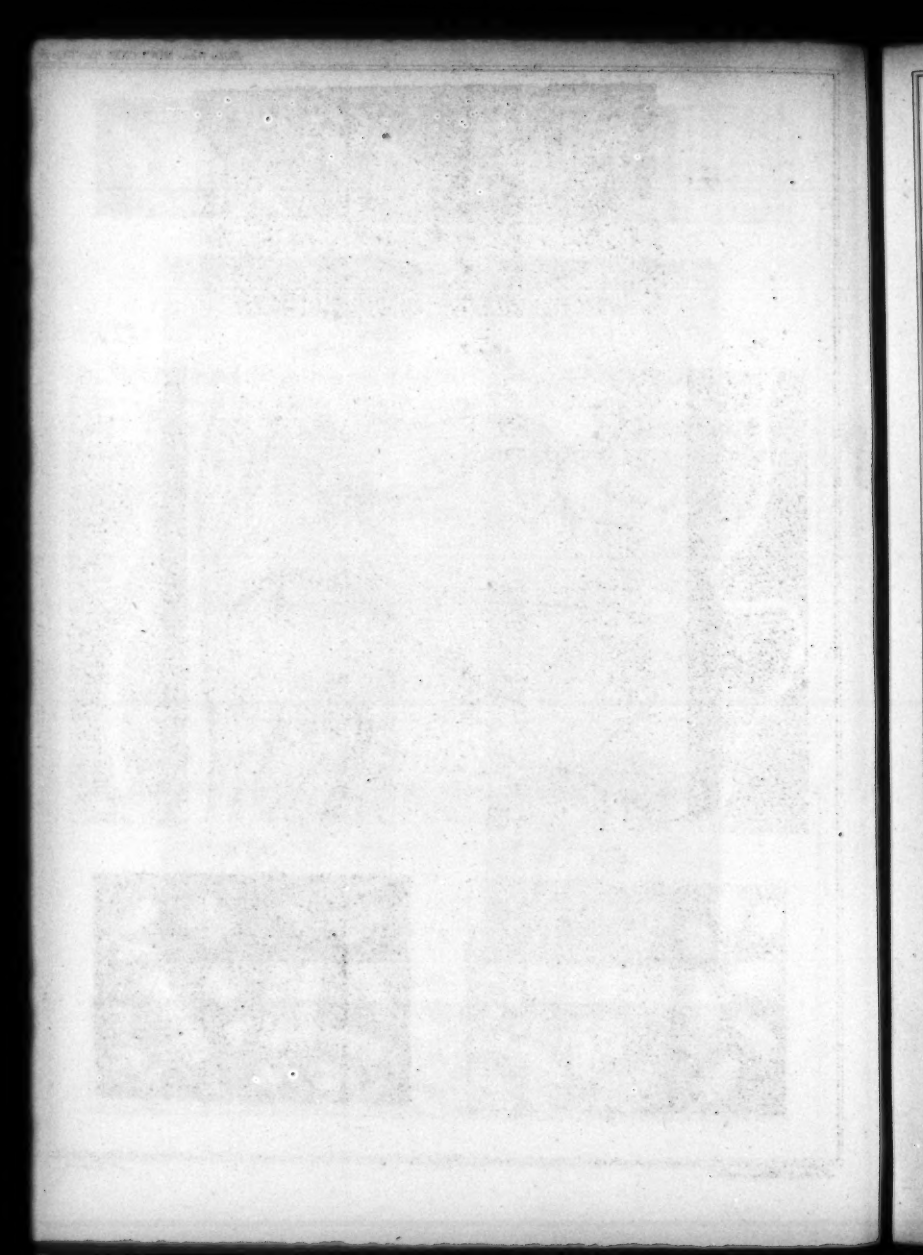

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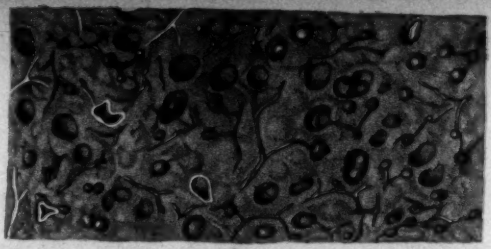
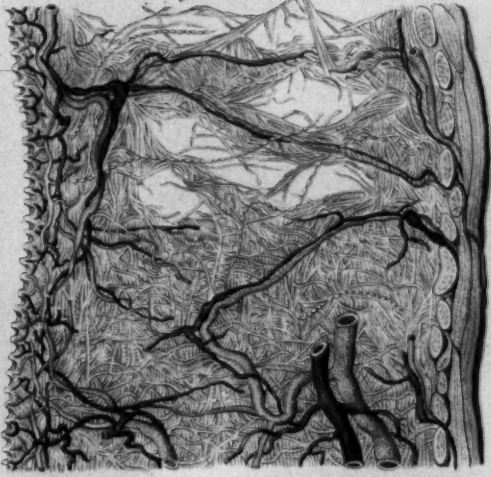


Fig. 3.

Fig. 2.



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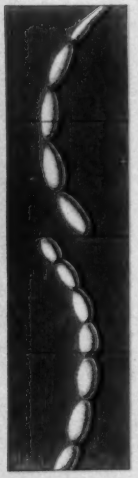

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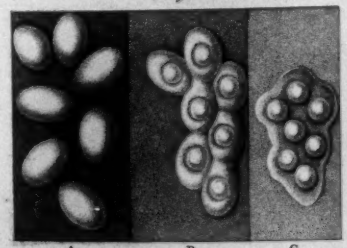

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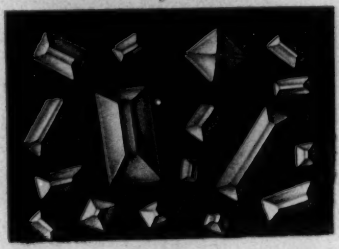
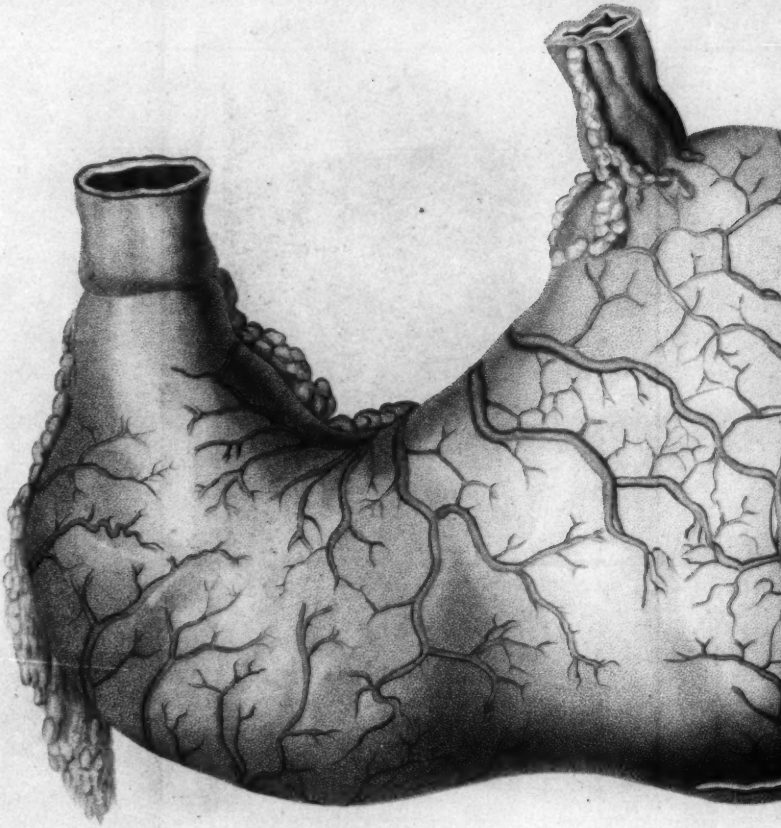
Fig. 6.





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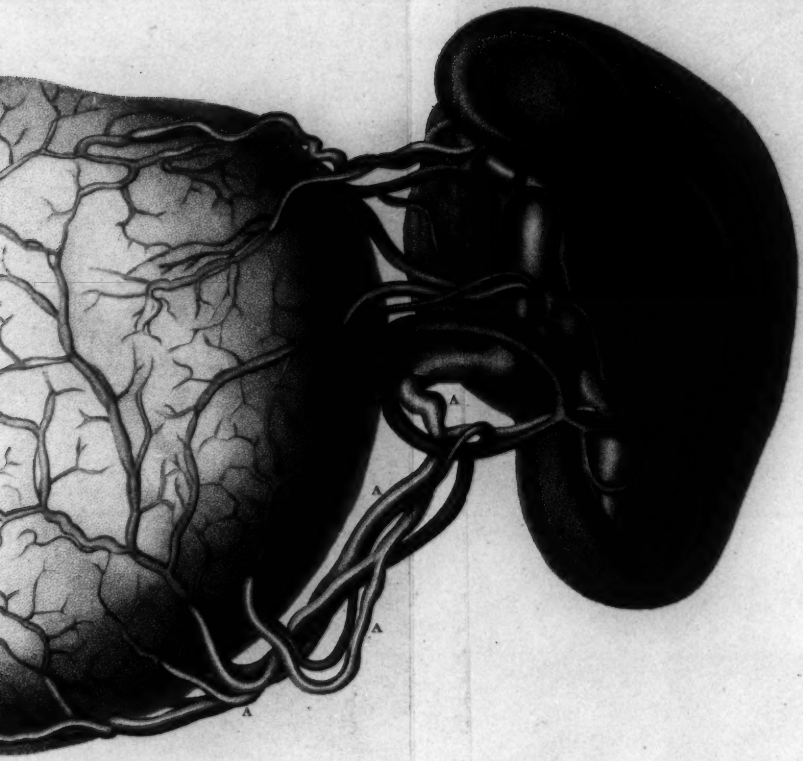
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Fig. 1.

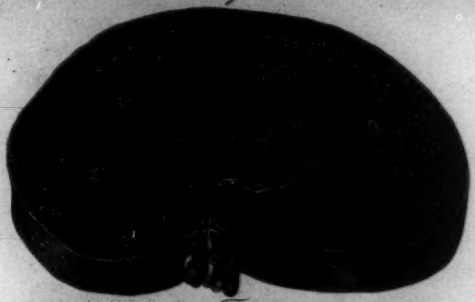
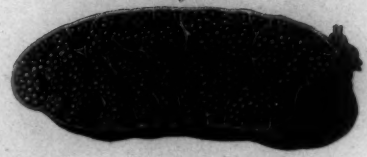


Fig. 2.

Fig. 3



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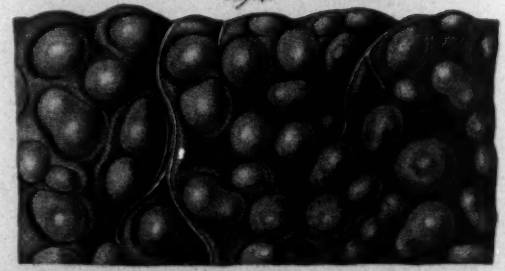

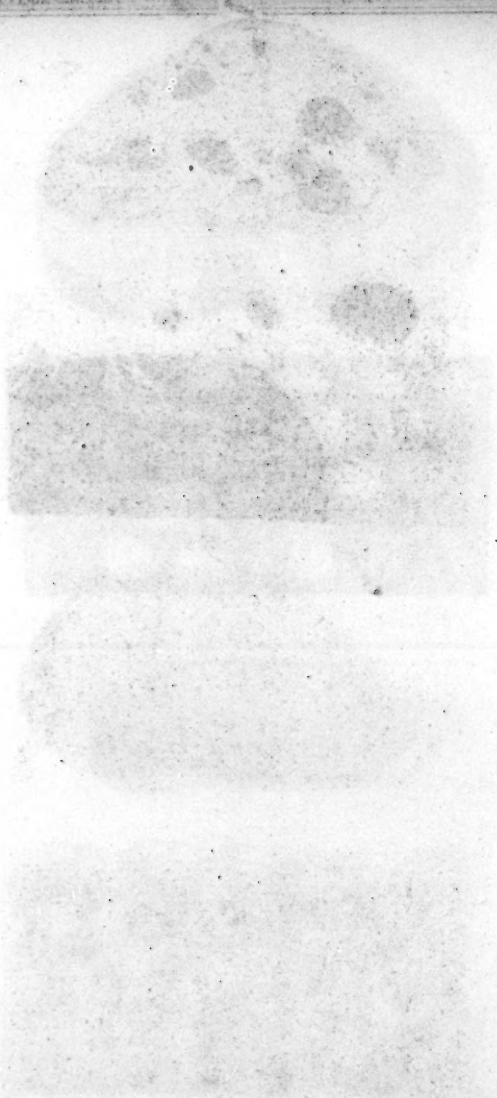

Fig. 1.

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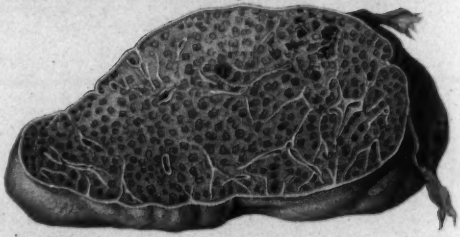

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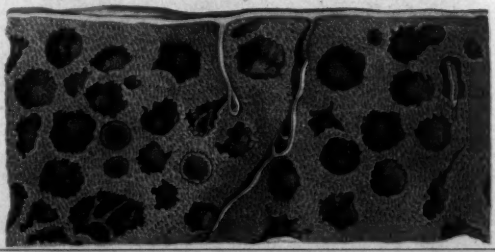


Fig. 1.

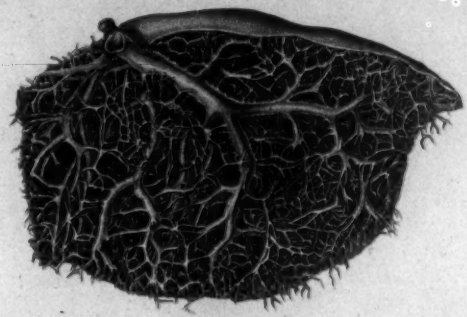
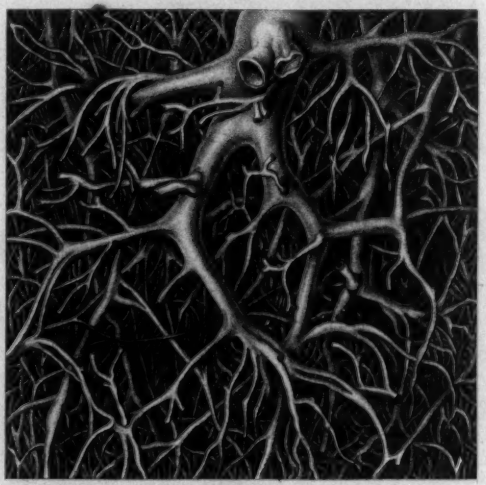


Fig. 2.



VI. *On two new compounds of Chlorine and Carbon, and on a new compound of Iodine, Carbon, and Hydrogen. By Mr.* FARADAY, *Chemical Assistant in the Royal Institution. Communicated by* W. T. BRANDE, *Esq. Sec. R. S. and Prof. Chem. R. I.*

Read December 21, 1820.

ONE of the first circumstances that induced Sir H. DAVY to doubt the compound nature of what was formerly called oxymuriatic acid gas, was the want of action of heated charcoal upon it; and considerable use of the same agent, and of the phenomena exhibited by it in different circumstances with chlorine, was afterwards made in establishing the simple nature of that body.

The true nature of chlorine being ascertained, it became of importance to form all the possible compounds of it with other elementary substances, and to examine them in the new view had of their nature. This investigation has been pursued with such success at different times, that very few elements remain uncombined with it; but with respect to carbon, the very circumstance which first tended to correct the erroneous opinions which, after SCHEELE's time, and before the year 1810, had gone abroad respecting its nature, proved an obstacle to the formation of its compounds; and up to the present time, the chlorides of carbon have escaped the researches of chemists.

That the difficulty met with in forming a compound of chlorine and carbon, was probably not owing to any want or

weakness of affinity between the two bodies, was pointed out by Sir H. DAVY; who, reasoning on the triple compound of chlorine, carbon, and hydrogen, concluded that the attraction of the two bodies for each other was by no means feeble; and the discovery of phosgene gas by Dr. DAVY, in which chlorine and carbon are combined with oxygen, was another circumstance strongly in favour of this opinion.

I was induced last summer to take up this subject, and have been so fortunate as to discover two chlorides of carbon, and a compound of iodine, carbon, and hydrogen, analogous in its nature to the triple compound of chlorine, carbon, and hydrogen, sometimes called chloric ether. I shall endeavour in the following pages to describe these substances, and give the experimental proofs of their nature.

If chlorine and olefiant gas be mixed together, it is well known that condensation takes place, and a colourless limpid volatile fluid is produced, containing chlorine, carbon, and hydrogen. If the volumes of the two gases are equal, the condensation is perfect. If the olefiant gas is in excess, that excess is left unchanged. But if the chlorine is in excess, the fluid becomes of a yellow tint, and acid fumes are produced. This circumstance alone proves, that chlorine can take hydrogen from the fluid; and on examination, I found it was without the liberation of any carbon or chlorine.

That the action thus began, might be carried to its utmost extent, some of the pure fluid (chloric ether) was put into a retort with chlorine, and exposed to sunshine. At the first instant of contact between the chlorine and the fluid, the latter became yellow; but when in the sun's rays, a few moments sufficed to destroy the colour both of the fluid and the chlorine,

heat being at the same time evolved. On opening the retort, there was no absorption, but it was found full of muriatic acid gas. This was expelled, and more chlorine introduced, and the whole again exposed to sun light: the colour again disappeared, and a few moist crystals were formed round the edge of the fluid. Chlorine being a third time introduced, and treated as before, it still removed more hydrogen; and now a sublimate of crystals lined the retort. Proceeding in this way until the chlorine exerted no farther action, the fluid entirely disappeared, and the results were, the dry crystalline substance, and muriatic acid gas.

A portion of olefiant gas was then mixed in a retort with eight or nine times its bulk of chlorine, and exposed to sun light. At first the fluid formed; but this instantly disappeared; the retort became lined with crystals, and the colour of the chlorine very much diminished.

On examining these crystals, I found they were the compound I was in search of; but before I give the proofs of their nature, I will describe the process by which this chloride of carbon can be obtained pure.

Perchloride of carbon.

A glass vessel was made in the form of an alembic head, but without the beak; the neck was considerably contracted, and had a brass cap with a stop-cock cemented on; at the top was a small aperture, into which a ground stopper fitted air tight. The capacity of the vessel was about 500 cubic inches. Being exhausted by the air-pump, it was nearly filled with chlorine; and being then placed over olefiant gas, and as much as could enter having passed in, the stop-cocks

were shut, and the whole left for a short time. When the fluid compound of chlorine and olefiant gas had formed and condensed on the sides of the vessel, it was again placed over olefiant gas, and, in consequence of the condensation of a large portion of the gases, a considerable quantity more entered. This was left, as before, to combine with part of the remaining chlorine, to condense, and to form a partial vacuum; which was again filled with olefiant gas, and the process repeated until all the chlorine had united to form the fluid, and the vessel remained full of olefiant gas. Chlorine was then admitted in repeated portions as before; consequently, more of the fluid formed; and ultimately a large portion was obtained in the bottom of the vessel, and an atmosphere of chlorine above it. It was now exposed to sun light. The chlorine immediately disappeared, and the vessel became filled with muriatic acid gas. Having ascertained that water did not interfere with the action of the substances, a small portion was admitted into the vessel which absorbed the muriatic acid gas, and then another atmosphere of chlorine was introduced. Again exposed to the light, this was partly combined with the carbon, and partly converted into muriatic acid gas; which, being as before absorbed by the water, left space for more chlorine. Repeating this action, the fluid gradually became thick and opaque from the formation of crystals in it, which at last adhered to the sides of the glass as it was turned round; and ultimately the vessel only contained chlorine with the accumulated gaseous impurities of the successive portions; a strong solution of muriatic acid, coloured blue from the solution of a little brass, and the solid substance.

I have frequently carried the process thus far in retorts;

and it is evident, that any conveniently formed glass vessel will answer the purpose. The admission of water during the process prevents the necessity of repeated exhaustion by the air-pump, which cannot be done without injury to the latter; but to have the full advantage of this part of the process, the gases should be as pure as possible, that no atmosphere foreign to the experiment may collect in the vessel.

In order to cleanse the substance, the remaining chlorine and muriatic acid were blown out of the vessel by a pair of bellows, introduced at the stoppered aperture, and the vessel afterwards filled with water, to wash away the muriatic acid and other soluble matters. Considerable care is then requisite in the farther purification of the chloride. It retains water, muriatic acid, and a substance, which I find to be a triple compound of chlorine, carbon, and hydrogen, formed from the cement of the cap; and as all these contain hydrogen, a small quantity of any one remaining with the chloride would, in analysis, give erroneous results. Various methods of purification may be devised, founded on the properties of the substance, but I have found the following the most convenient. The substance is to be washed from off the glass, and poured with the water into a jar; a little alcohol will remove the last portions which adhere to the glass; and this when poured into the water will precipitate the chloride, and the whole will fall to the bottom of the vessel. Then having decanted the water, the chloride is to be collected on a filter, and dried as much as may be by pressure between folds of bibulous paper. It should next be introduced into a glass tube, and sublimed by a spirit lamp: the pure substance with water will rise at first, but the last portions will be partially decom-

posed, muriatic acid will be liberated, and charcoal left. The sublimed portion is then to be dissolved in alcohol, and poured into a weak solution of potash, by which the substance is thrown down, and the muriatic acid neutralized and separated: then wash away the potash and muriate by repeated affusions of water, until the substance remains pure; collect it on a filter, and dry it, first between folds of paper, and afterwards by sulphuric acid in the exhausted receiver of the air-pump.

It will now appear as a white pulverulent substance; and if perfectly pure, will not, when a little of it is sublimed in a tube, leave the slightest trace of carbon, or liberate any muriatic acid. A small portion of it dissolved in ether, should give no precipitate with nitrate of silver. If it be not quite pure, it must be re-sublimed, washed, and dried until it is pure.

This substance does not require the direct rays of the sun for its formation. Several tubes were filled with a mixture of one part of olefiant gas with five or six parts of chlorine, and placed over water in the light of a dull day; in two or three hours there was very considerable absorption, and crystals of the substance were deposited on the inside of the tubes. I have also often observed the formation of the crystals in retorts in common day light.

A retort being exhausted, had 19 cubic inches of olefiant gas introduced, and 24.75 cubic inches of chlorine: as soon as the condensation occasioned by the formation of the fluid had taken place, 21.5 cubic inches more of chlorine were passed in, and the retort set aside in a dark place for two days. At the end of that time muriatic acid gas and the solid

chloride had formed, but the greater part of the fluid remained unchanged. Hence, it will form even in the dark, by length of time.

I tried to produce the chloride by exposure of the two gases in tubes over water to strong lamp light for two or three hours, but could not succeed.

The perchloride of carbon, when pure, is immediately after fusion, or sublimation, a transparent colourless substance. It has scarcely any taste. Its odour is aromatic, and approaching to that of camphor. Its specific gravity is as nearly as possible 2. Its refractive power is high, being above that of flint glass (1.5767). It is very friable, easily breaking down under pressure; and when scratched, has much of the feel and appearance of white sugar. It does not conduct electricity.

The crystals obtained by sublimation and from solutions of the substance in alcohol and ether, are dendritical, prismatic, or in plates; the varieties of form, which are very interesting, are easily ascertained, and result from a primitive octoëdron.

It volatilizes slowly at common temperatures, and passes, in the manner of camphor, towards the light. If warmed, it rises more rapidly, and then forms fine crystals: when the temperature is farther raised, it fuses at 390° F. and boils at 360°, under atmospheric pressure. When condensed again from these rapid sublimations, it concretes in the upper part of the tube or vessel containing it, in so transparent and colourless a state, that it is difficult, except from its high refractive power, to perceive where it is lodged. As the crust it forms becomes thicker, it splits, and cracks like sublimed camphor; and in a few minutes after it is cold, is white, and

nearly opaque. If the heat be raised still higher, as when the substance is passed through a red hot tube, it is decomposed, chlorine is evolved, and another chloride of carbon, which condenses into a fluid, is obtained. This shall be described presently.

It is not readily combustible; when held in the flame of a spirit lamp, it burns with a red flame, emitting much smoke and acid fumes; but when removed from the lamp, combustion ceases. In the combustion that does take place in the lamp, the hydrogen of the alcohol, by combining with the chlorine of the compound, performs the most important part; nevertheless, when the substance is heated red in an atmosphere of pure oxygen, it sometimes burns with a brilliant light.

It is not soluble in water at common temperatures; or only in very small quantity. When a drop or two of the alcoholic solution is poured into a large quantity of water, it renders it turbid from the deposition of the substance. It does not appear that hot water dissolves more of it than cold water.

It dissolves in alcohol with facility, and in much greater quantity with heat than without. A saturated hot solution crystallizes as it cools, and the cold solution also gives crystals by spontaneous evaporation. When poured into water, the chloride is precipitated, and falls to the bottom in flakes. If burnt, the flame of the alcohol is brightened by the presence of the substance, and fumes of muriatic acid are liberated. Solution of nitrate of silver does not produce any turbidness in it, unless it be in such quantity that the water throws down the substance; but no chloride of silver is formed.

It is much more soluble in ether than in alcohol, and more

so in hot than in cold ether. The hot solution deposits crystals as it cools; and the crystallization of a cold solution, when evaporated on a glass plate, is very beautiful. This solution is not precipitated by water, unless the ether has previously been dried, and then water occasions a turbidness. Nitrate of silver does not precipitate it. When burned, muriatic acid fumes are liberated, but the greater part of the chloride remains in the capsule.

It is soluble in the volatile oils, and on evaporation is again obtained in crystals. It is also readily soluble in fixed oils. The solutions when heated liberate muriatic acid gas, and the oil becomes of a dark colour, as if charred.

Solutions of the acids and alkalies do not act with any energy on the substance. When boiled with solutions of pure potash and soda, it rises and condenses in the upper part of the vessel; and though it be brought down to the alkali many times, and re-boiled, still the alkali, when examined, is not found to contain any chlorine, nor is any change produced. Ammonia in solution is also without action upon it. These solutions do not appear to dissolve more of it than pure water.

Muriatic acid in solution does not act at all upon it. Strong nitric acid boiled upon it dissolves a portion, but does not decompose it: as it cools, part of the chloride is deposited unaltered, and the concentrated acid, when diluted, lets more fall down. The diluted portion being filtered, and tested with nitrate of silver, gives no precipitate. It does not appear to be either soluble in, or acted upon, by concentrated sulphuric acid. It sinks slowly in the acid, and, when heated,

is converted into vapour, which, rising through the acid, condenses in the upper part of the tube.

It is not acted upon by oxygen at temperatures under a red heat. A mixture of oxygen and the vapour of the substance would not inflame by a strong electric spark, though the temperature was raised by a spirit-lamp to about 400°. When oxygen mixed with the vapour of the substance is passed through a red-hot tube, there is decomposition, and mixtures of chlorine, carbonic oxide, carbonic acid, and phosgene gases are produced. A portion of the chloride was heated with peroxide of mercury in a glass tube over mercury; as soon as the oxide had given off oxygen, and the heat had risen so high as to soften the glass considerably, the vapour suddenly detonated with the oxygen with bright inflammation. The substances remaining were oxygen, carbonic acid, and calomel; and I believe there was no decomposition or action, until so much mercury had risen in vapour as to aid the oxygen by a kind of double affinity in decomposing the chloride of carbon.

Chlorine produces no change on the substance, either by exposure to light or heat.

When iodine is heated with it at low temperatures, the two substances melt and unite, and there is no farther action. When heated more strongly in vapour, the iodine separates chlorine, reducing the perchloride to the fluid protochloride of carbon, and chloriodine is produced. This dissolves, and if no excess of iodine be present, the whole remains fluid at common temperatures. When water is added, it generally liberates a little iodine; and on heating the solution, so as to

drive off all free iodine, and testing by nitrate of silver, chloride and iodide of silver are obtained.

Hydrogen and the vapour of the substance would not inflame at the temperature of 400° F. by strong electrical sparks; but when the mixture was sent through a red hot tube, the chloride was decomposed, and muriatic acid gas and charcoal produced.

The vapour of the perchloride of carbon readily detonates by the electric spark with a mixture of oxygen and hydrogen gases; but the gaseous results are very mixed and uncertain, from the near equipoise of affinities that exist among the elements.

Sulphur readily unites to it when melted with it, and the mixture crystallizes on cooling into a yellowish mass. When heated more strongly, the substance rises unchanged, and leaves the sulphur unaltered; but when the mixed vapours are raised to a still higher temperature, chloride of sulphur and proto-chloride of carbon are formed. Sometimes there are appearances as if a carburet of sulphur were formed, but of this I have not satisfied myself.

Phosphorus at low temperatures melts and unites with the substance, without any decomposition. If heated in the vapour of the substance, but not too highly, it takes away chlorine, and forms the proto-chlorides of phosphorus and carbon. If heated more highly, it frequently inflames in the vapour with a brilliant combustion, and abundance of charcoal is deposited. Sometimes I have had the charcoal left in films stretching across the tubes, and occupying the space where the flame passed. The appearance is then very beautiful.

When phosphorus is heated with the vapour of the substance over mercury, so as not to inflame in it, there is generally a small portion of muriatic acid gas formed. If great care be taken, this is in very minute quantity; and its variable proportion sufficiently shows, that the hydrogen which forms it does not come from the substance. I am induced to believe that it is derived from moisture adhering to the phosphorus. The action of iodine on phosphorus shows, that it is very difficult to dry the latter substance perfectly.

A stick of phosphorus put into the alcoholic or etherial solution of the perchloride did not exert any action upon it.

Charcoal heated in the vapour of the substance appears to have no action upon it.

Most of the metals decompose it at high temperatures. Potassium burns brilliantly in the vapour, depositing charcoal, and forming chloride of potassium. Iron, zinc, tin, copper and mercury, act on it at a red heat, forming chlorides of those metals, and depositing charcoal; and when the experiments are made with pure substances, and very carefully, no other results are obtained. Some of the substance was passed over iron turnings heated in a glass tube. At the commencement of the sublimation of the chloride through the hot iron, the common air of the vessels was expelled, and received in different tubes; but before one-third of the substance had been passed, all liberation of gas ceased, and the remainder was decomposed by the iron, without the production of any gaseous matters. The different portions of air that were thrown out being examined, the first proved to be common air, and the last carbonic oxide. This had resulted, probably, from the action of the chlorine on the lead of the glass tube.

An evident action had taken place, and the oxygen evolved, meeting with the liberated carbon, would produce the carbonic oxide. This experiment has been repeated several times with the same results.

When the perchloride of carbon is heated with metallic oxides, different results are produced according to the proportions of oxygen in the oxides. The peroxides, as of mercury, copper, lead, and tin, produce chlorides of those metals, and carbonic acid; and the protoxides, as those of zinc, lead, &c. produce also chlorides; but the gaseous products are mixtures of carbonic acid and carbonic oxide. I have frequently perceived the smell of phosgene gas on passing the chloride over oxide of zinc; and as the substance easily liberates chlorine at high temperatures, it will be readily seen how a small portion of that gas may be formed. It also happens, sometimes, that the protoxides become blackened from the deposition of charcoal.

When the vapour of the chloride is passed over lime, baryta, or strontia, heated red hot, a very vivid combustion is produced. The oxygen and the chlorine change places, and both the metals and the carbon are burnt. Chlorides are produced, carbonic acid is formed and absorbed by the undecomposed parts of the earths, and carbon is deposited. In these experiments no carbonic oxide is produced. When passed over magnesia, there is no action on the earth, but the perchloride of carbon is converted by the heat into protochloride.

In these experiments with the oxides no trace of water could be perceived.

Having thus far described the properties of the substance,

I shall now give the reasons which induce me to consider it a true chloride of carbon, and shall endeavour to assign its composition. My first object was to ascertain whether hydrogen existed in it, or not. When phosphorus is heated in it, a small quantity of muriatic acid is generally formed; but doubt arises as to the cause of its production, from the circumstance that the phosphorus, as already mentioned, may be the source of the hydrogen. When potassium is heated in the vapour of the substance, there is generally a small expansion of volume, and inflammable gas produced; but it is very difficult to cleanse potassium both from naptha and an adhering crust of moist potash; and either of these, though in extremely minute quantities, would give fallacious results.

A more unexceptionable experiment made with iron, has been already described; and the inferences from it are against the presence of hydrogen in the compound.

Some of the substance in vapour was electrized over mercury, by having many hundred sparks passed through it. Calomel was formed, and carbon deposited. A very minute bubble of gas was produced, but it was much too small to interfere with the conclusions drawn respecting the binary nature of the compound; and was probably caused by air that had adhered to the sides of the tube when the mercury was poured in.

The most perfect demonstration that the body contains no hydrogen, and indeed of its nature altogether, is obtained from the circumstances which attend its formation. When the fluid compound of chlorine and olefiant gas is acted on by chlorine and solar light in close vessels, although the whole of the chlorine disappears, yet there is no change of

volume, its place being occupied by muriatic acid gas. Hence, as muriatic acid gas is known to consist of equal volumes of chlorine and hydrogen, combined without condensation, it is evident that half the chlorine introduced into the vessel has combined with the elements of the fluid, and liberated an equal volume of hydrogen; and as, when the chloride is perfectly formed, it condenses no muriatic acid gas, a method, apparently free from all fallacy, is thus afforded of ascertaining its nature.

I have made many experiments on given volumes of chlorine and olefiant gases. A clean dry retort was fitted with a cap and stop-cock. Its capacity was 25.25 cubic inches. Being exhausted by the air-pump, it was filled with nitrogen (24.25 cubic inches being required), and being again exhausted, 5 cubic inches of olefiant gas, and 10 cubic inches of chlorine were introduced. It was then set aside for half an hour, that the fluid compound might form, and afterwards being placed again over a jar of chlorine, 19.25 cubic inches entered; so that the condensation had been as nearly as possible 10 cubic inches, or twice the volume of the olefiant gas (barometer 29.1 inches). It was now placed for the day (October 18th.) in the rays of the sun; but the weather was not very fine. In the evening the solid crystalline substance had formed in abundance, and very little fluid remained. When placed over chlorine, not the slightest change in volume had been produced. The stop-cock was now opened under mercury, and a small portion of the metal having entered, it was agitated in the retort, to absorb the chlorine; the neck of the retort was left open under the mercury all night, and the whole agitated from time to time. Next morning (barometer 29.6) the mercury

which had entered, being passed into the neck of the retort, stood at a certain mark 6 inches above the level of the mercury in the trough, occupying 1.25 cubic inches, and leaving 24 cubic inches filled by the expanded muriatic acid gas and nitrogen. These volumes corrected to the pressure of 29.1 inches, give 5.78 cubic inches for the chlorine absorbed, and 19.47 cubic inches for the muriatic acid gas, &c. These absorbed by water left 1.2 cubic inches of nitrogen; so that the gases in the retort, after the action of solar light, were

Muriatic acid gas -	18.27 cubic inches.
Chlorine - -	5.78
Nitrogen, &c. -	1.2

and before that action,

Chlorine - -	29.25 cubic inches.
Olefiant gas -	5.
Nitrogen - -	1.

Hence 23.47 cubic inches of chlorine had disappeared, and 9.13 of these had entered into combination with an equal volume of 9.13 cubic inches of hydrogen liberated from the 5 cubic inches of olefiant gas, to form muriatic acid; and, consequently, 14.34 cubic inches of chlorine remained combined with the carbon of the 5 cubic inches of olefiant gas. Here, the volume of chlorine actually employed is not quite five times that of the olefiant gas, nor the volume of muriatic acid gas produced, equal to four times that of the olefiant gas; but they approximate; and when it is remembered that the conversion was not quite perfect, and that the gases used would inevitably contain a slight portion of impurity, the causes of the deficiency can easily be understood.

In other experiments made in the same way, but with

smaller quantities, more accurate results were obtained. 1 cubic inch of olefiant gas with 12.25 cubic inches of chlorine, produced by the action of light 3.67 cubic inches of muriatic acid gas, 4.963 of the chlorine having been used. 1.4 cubic inch of olefiant gas with 12.5 cubic inches of chlorine produced 5.06 cubic inches of muriatic acid gas, 6.7 cubic inches of chlorine having been used. Other experiments gave very nearly the same results; and I have deduced from them, that 1 volume of olefiant gas requires 5 volumes of chlorine for its conversion into muriatic acid and chloride of carbon; that 4 volumes of muriatic acid gas are formed; that 3 volumes of chlorine combine with the 2 volumes of carbon in the olefiant gas to form the solid crystalline chloride; and that, when chlorine acts on the fluid compound of chlorine and olefiant gas, for every volume of chlorine that combines, an equal volume of hydrogen is separated.

I have endeavoured to verify these proportions by analytical experiments. The mode I adopted was, to send the substance in vapour over metals and metallic oxides at high temperatures. Considerable care is requisite in such experiments; for if the process be carried on quickly, a portion of fluid chloride of carbon is formed, and escapes decomposition. The following are two results, from a number of experiments agreeing well with each other.

5 grains were passed over peroxide of copper in an iron tube, and the gas collected over mercury; it amounted to 3.9 cubic inches, barometer 29.85; thermometer 54° F. Of these nearly 3.8 cubic inches were carbonic acid, and rather more than ,1 of a cubic inch was carbonic oxide. These are nearly equal to ,5004 of a grain of carbon. Hence, 100

of the chloride would give 10 of carbon nearly, but by calculation 100 should give 10.19. The difference is so small, as to come within the limits of errors in experiment.

5 grains were passed over peroxide of copper in a tube made of green phial glass, and the chlorine estimated in the same manner as before. 17.7 grains of chloride of silver were obtained equal to 4.86 grains of chlorine. This result approaches much nearer to the calculated result than the former; but there had still been action on the tube, and a minute portion of the substance had passed undecomposed, and condensed at the opposite end of the tube in crystals.

Experiments made by passing the perchloride over hot lime or barytes, promise to be more accurate and easy of performance. In the mean time, the above analytical results will perhaps be considered as strong corroboration of the opinion of the nature of the compound, deduced from the synthetical experiments; and the composition of the perchloride of carbon will be

3	proportions of chlorine	= 100.5
2	ditto carbon	= 11.4
		111.9

Proto-chloride of carbon.

Having said so much on the nature of the perchloride of carbon, I shall have less occasion to dwell on the proofs that the compound I am about to describe, is also a binary combination of carbon and chlorine.

When the vapour of the perchloride of carbon is heated to dull redness, chlorine is liberated, and a new compound of that element and carbon is produced. This is readily shown

by heating the bottom of a small glass tube, containing some of the perchloride, in a spirit lamp. The substance at first sublimes, but as the vapour becomes heated below, it is gradually converted into proto-chloride, and chlorine is evolved.

It is not without considerable precaution that the proto-chloride of carbon can be obtained pure; for though passed through a great length of heated tube, part of the perchloride frequently escapes decomposition. The process I have adopted is the following: some of the perchloride is introduced into the closed end of a tube, and the space above it, for ten or twelve inches, filled with small fragments of rock crystal; the part of the tube beyond this is then bent up and down two or three times, so that the angles may form receivers for the new compound; then heating the tube and crystal to bright redness, and dipping the angles in water, the perchloride is slowly sublimed by a spirit lamp, and, on passing into the hot part of the tube, is decomposed; a fluid passes over, which is condensed in the angles of the tube, and chlorine is evolved: part of the gas escapes, but the greater portion is retained in solution by the fluid, and renders it yellow. Having proceeded thus far, by the careful application of a lamp and blow-pipe, the bent part of the tube may be separated from that within the furnace, and the end closed, so as to form a small retort; and on distilling the fluid four or five times from one angle to the other, all the chlorine may be driven off without any loss of the substance, and it becomes limpid and colourless. It still, however, always contains some perchloride, which has escaped decomposition, and, to separate this, I have boiled the fluid until the tube was nearly full of its vapour, and then closing

the end that still remained open, by a lamp and blow-pipe, have afterwards left the whole to cool. It is then easy by collecting all the fluid into one end of the tube, and introducing that end through a cork into a receiver, under which a very small flame is burning, to distil the whole of the fluid at a temperature very little above that of the atmosphere. The solid chloride being less volatile does not rise so soon, and the pure proto-chloride collects at the external end of the tube. To ascertain its purity, a drop may be placed on a glass plate; it will immediately evaporate, and if it contains perchloride, that substance will be left behind; otherwise, no trace will remain on the glass. The presence or absence of free chlorine may be ascertained by dissolving a little of the fluid in alcohol or ether, and testing by nitrate of silver.

The pure proto-chloride of carbon is a highly limpid fluid, and perfectly colourless. Its specific gravity is 1.5326. It is a non-conductor of electricity. I am indebted to Dr. WOLLASTON for the determination of the refractive power of this chloride, and for the approximation to the refractive power given of the perchloride. In the present case it is 1.4875, being very nearly that of camphor. It is not combustible except when held in a flame, as of a spirit lamp, and then it burns with a bright yellow light, much smoke, and fumes of muriatic acid.

It does not become solid at the zero of Fahrenheit's scale. When its temperature is raised under the surface of water to between 160° and 170°, it is converted into vapour, and remains in that state until the temperature is lowered. When heated more highly, as by being passed over red hot rock crystal in a glass tube, a small portion is always decomposed; nearly all the fluid may, however, be condensed again, but

it passes slightly coloured, and the tube and crystal are blackened on the surface by charcoal. I am uncertain whether this decomposition ought not to be attributed rather to the action of the glass at this high temperature than to the heat alone.

It is not soluble in water, but remains at the bottom of it in drops, for many weeks, without any action.

It is soluble in alcohol and ether, and the solutions burn with a greenish flame, evolving fumes of muriatic acid.

It is soluble in the volatile and fixed oils. The volatile oils containing it burn with the emission of fumes of muriatic acid. When the solutions of it in the fixed oils are heated, they do not blacken or evolve fumes of muriatic acid. It is therefore probable, that when this happens with the solution of the perchloride in fixed oils, it is from its conversion by the heat into proto-chloride and the liberation of chlorine.

It is not soluble in alkaline solutions, nor do they act on it in some days. Neither is it at all soluble in, or affected by, strong nitric, muriatic, or sulphuric acids.

Solutions of silver do not act on it.

Oxygen decomposes it at high temperatures, forming carbonic oxide, or acid, and liberating chlorine.

Chlorine dissolves in it in considerable quantity, but has no farther action, or only a very slow one in common day light; on exposure to solar light, a different result takes place. I have only had two days, and those in the middle of November, on which I could expose the proto-chloride of carbon in atmospheres of chlorine to solar light; and hence the conversion of the whole of the proto-chloride was not perfect; but at the end of those two days the retorts containing the

substances were lined with crystals, which, on examination under the microscope, proved to be quadrangular plates, resembling those of the perchloride of carbon. There were also some rhomboidal crystals here and there. After the formation of these crystals, there was considerable absorption in the retort; hence chlorine had combined; and the gas which remained was chlorine unmixed with any thing else, except a slight impurity. The solid body, on examination, was found to be volatile, soluble in alcohol, precipitable by water, and had the smell and other properties of perchloride of carbon. Hence, though heat in separating chlorine from the perchloride of carbon produces its decomposition, light occasions its reproduction.

It dissolves iodine very readily, and forms a brilliant red solution, similar in colour to that made by putting iodine into sulphuret of carbon, or chloric ether. It does not exert any farther action on iodine at common temperatures.

An electric spark passed through a mixture of the vapour of the chloride with hydrogen, does not cause any detonation, but when a number are passed, the decomposition is gradually effected, and muriatic acid is formed. When hydrogen and the vapour of the proto-chloride are passed through a red hot tube, there is a complete decomposition effected, muriatic acid gas being formed, and charcoal deposited. The mixed vapour and gas burn with flame as they arrive in the hot part of the tube. The vapour of the proto-chloride detonates readily by the electric spark with a mixture of oxygen and hydrogen gases, and a complete decomposition is effected. It will not detonate with the vapour of water.

Sulphur and phosphorus both dissolve in it, but exert no

decomposing action at temperatures at, or below, the boiling point of the chloride. The hot solution of sulphur becomes a solid crystalline mass by cooling. Phosphorus decomposes it at a red heat.

Its action on metals is very similar to that of the perchloride. When passed over them at a red heat, it forms chlorides, and liberates charcoal. Potassium does not act on it immediately at common temperatures; but, when heated in its vapour, burns brilliantly, and deposits charcoal.

When passed over heated metallic oxides, chlorides of the metals are formed, and carbonic oxide, or carbonic acid, according to the state of oxidation of the metal. When its vapour is transmitted over heated lime, baryta, or strontia, the same brilliant combustion is produced as with the perchloride.

Whilst engaged in analyzing this chloride of carbon, for the purpose of ascertaining the proportions of its elements, I endeavoured, at first, to find how much chlorine was liberated from a certain weight of perchloride during its conversion into proto-chloride, and for this purpose distilled the perchloride through red hot tubes into solution of nitrate of silver, receiving the gas into tubes filled with and immersed in the same solution; but I could never get accurate results in this way, from the difficulty of producing a complete decomposition, and also from the formation of chloric acid. 5 grains of perchloride distilled in this manner gave 4.8 grains of chloride of silver, which are equivalent to 1.06 grains of chlorine; but some of the chloride evidently passed undecomposed, and crystallized in the tube.

2.7 grains of the pure proto-chloride were passed over red

hot pure baryta in a glass tube: a very brilliant combustion with flame took place, chloride of barium and carbonic acid were produced, and a little charcoal deposited. When the tube was cold, the barytes was dissolved in nitric acid, and the chlorine precipitated by nitrate of silver. 9.4 grains of dry chloride of silver were obtained = 2.32 grains of chlorine.

Other experiments were made with lime, which gave results very near to this, the quantity of chloride being rather less.

3 grains of pure proto-chloride were passed over peroxide of copper heated red hot in an iron tube, and the gas received over mercury. 3.5 cubic inches of carbonic acid gas came over mixed with ,1 of a cubic inch of common air. These 3.5 cubic inches are nearly equal to .449 of a grain of carbon.

These experiments indicate the composition of the fluid chloride of carbon to be 1 proportion of chlorine and 1 of carbon, or 93.5 of the former and 5.7 of the latter. The difference between these theoretical numbers, and the results of the experiments, is not too great to have arisen from errors in working on such small quantities of the substance.

A mixture of equal volumes of oxygen and hydrogen was made, and 2 volumes of it detonated with the vapour of the proto-chloride in excess over mercury by the electric spark. The expansion was very nearly to 4 volumes; of these, 2 were muriatic acid, and the rest pure carbonic oxide: and calomel had been formed, its presence being ascertained by potash. Hence it appears, that 1 volume of hydrogen and half a volume of oxygen had decomposed 1 proportion of the proto-chloride, forming the two volumes of muriatic

acid gas and 1 volume of carbonic oxide; and that at the intense temperature produced within the tube by the inflammation, the rest of the oxygen and the mercury had decomposed a farther portion of the substance, giving rise to the second volume of the carbonic oxide, and to the calomel.

A mixture of 2 volumes of hydrogen and 1 volume of oxygen was made, and 3 volumes of it detonated with the vapour, as before. After cooling, the expansion was to 6 volumes; 4 of which were muriatic acid, and 2 carbonic oxide. There was no action on the mercury in this experiment. Again, 5 volumes of the same mixture being detonated with the vapour of the substance, expanded to 9.75 volumes, of which 6.25 were absorbed by water and were muriatic acid, and 3.5 were carbonic oxide mixed with a very small portion of air introduced along with the fluid chloride. These experiments, I think, establish the composition of the proto-chloride of carbon, and prove that it contains 1 proportion of each of its elements.

From a consideration of the proportions of these two chlorides of carbon, it seems extremely probable that another may exist, composed of 2 proportions of chlorine combined with 1 of carbon. I have searched assiduously for such a compound, but am undecided respecting its production. When the fluid proto-chloride was exposed with chlorine to solar light, crystals were formed, as before described. The greater number of these were certainly the perchloride first mentioned in this paper; but when the retort was examined by a microscope, some rhomboidal crystals were observed here and there among those of the usual dendritic and square forms.

These may, perhaps, be the real perchloride; but I had not time, before the season of bright sunshine passed away, to examine minutely what happens in these circumstances; and must defer this, with many other points, till the next year brings more favourable weather.

Compound of Iodine, Carbon, and Hydrogen.

The analogy which exists between chlorine and iodine, naturally suggested the possible existence of an iodide of carbon, and the means which had succeeded with the one element, offered the best promise of success with the other.

Iodine and olefiant gas were put in various proportions into retorts, and exposed to the sun's rays. After a while, colourless crystals formed in the vessels, and a partial vacuum was produced. The gas in the vessels being then examined, was found to contain no hydriodic acid, but only pure olefiant gas. Hence, the effect had been simply to produce a compound of the iodine with the olefiant gas.

The new body formed was obtained pure by introducing a solution of potash into the retort, which dissolved all the free iodine; the substance was then collected together and dried. It is a solid white crystalline body, having a sweet taste and aromatic smell. It sinks readily in sulphuric acid of specific gravity 1.85. It is friable; is not a conductor of electricity. When heated, it first fuses, and then sublimes without any change. Its vapour condenses into crystals, which are either prismatic, or in plates. On becoming solid after fusion, it also crystallizes in needles. The crystals are transparent. When highly heated it is decomposed, and iodine evolved. It is not readily combustible; but when

held in the flame of a spirit lamp, burns, diminishing the flame, and giving off abundance of iodine, and some fumes of hydriodic acid. It is insoluble in water, or in acid and alkaline solutions. It is soluble in alcohol and ether, and may be obtained in crystals from these solutions. The alcoholic solution is of a very sweet taste, but leaves a peculiarly sharp biting sensation on the tongue.

Sulphuric acid does not dissolve it. When heated in the acid to between 300° and 400°, the compound is decomposed, apparently by the heat alone; and iodine and a gas, probably olefiant gas, are liberated. Solution of potash acts on it very slowly, even at the boiling point, but does gradually decompose it.

This substance is evidently analogous to the compound of olefiant gas and chlorine, and remarkably resembles it in the sweetness of its taste, though it differs from it in form, &c. It will with that body form a new class of compounds, and they will require names to distinguish them. The term chloric ether, applied to the compound of olefiant gas and chlorine, did not at any time convey a very definite idea, and the analogous name of iodic ether, would evidently be very improper for a solid crystalline body heavier than sulphuric acid. Mr. Brande has suggested the names of hydriodide of carbon, and hydrochloride of carbon, for these two bodies. Perhaps as their general properties range with those of the combustibles, whilst the specific nature of the compound is decided by the supporter of combustion which is in combination, the terms of hydro-carburet of chlorine, and hydro-carburet of iodine, may be considered as appropriate for them.

As yet I have not succeeded in procuring an iodide of carbon, but I intend to pursue these experiments in a brighter season of the year, and expect to obtain this compound.

VII. *An account of the comparison of various British Standards of linear measure.* By Capt. HENRY KATER, *F. R. S. &c.*

Read January 18th, 1821.

THE Commissioners appointed to consider the subject of Weights and Measures, recommended in their First Report "for the legal determination of the standard yard, that "which was employed by General Roy in the measurement "of a Base on Hounslow Heath, as a foundation for the Tri-"gonometrical operations that have been carried on by the "Ordnance throughout the country." In consequence of this determination, it became necessary to examine the standard to which the Report alludes, with the intention of subsequently deriving from it a scale of feet and inches.

On referring to the Philosophical Transactions for 1785, it may be seen in "an Account of the Measurement of a Base "on Hounslow Heath," that a brass scale, the property of General Roy (and now in the possession of HENRY BROWNE, Esq. F. R. S.), was taken to the apartments of the Royal Society, and being there, with the assistance of Mr. RAMSDEN, compared with their standard (both having remained together two days previous to the comparison), the extent of 8 feet taken from the Society's standard, and applied to General Roy's scale, was found to reach exactly to 86 inches, at the temperature of 65°.

It afterwards appears that points, at the distance of 40

inches from each other, were laid off on a large plank from General ROY's scale, the whole length being 20 feet; and by means of this plank the length of the glass rods was determined, with which the base on Hounslow Heath was measured.

In the Philosophical Transactions for 1795, it is stated, that Mr. RAMSDEN compared *his* brass standard with that belonging to the Royal Society, after they had remained together about 24 hours, when "they were found to be precisely of the same "length." Brass points were then inserted in the upper surface of a cast iron triangular bar of 21 feet in length, from Mr. RAMSDEN's standard, at the distance of 40 inches from each other, the whole length of 20 feet being laid off on those points in the temperature of 54°.

By means of this bar, the length of the hundred feet steel chain was determined with which the base on Hounslow Heath was re-measured, and was found to be only about $2\frac{3}{4}$ inches greater than the measurement with the glass rods.

The standard scale used by Mr. RAMSDEN in laying off the points on the iron bar, is, it seems, no longer to be found; but from the declared equality of both this and General ROY's standard with that of the Royal Society, and the near agreement of the two separate measurements of the base with the glass rods and with the steel chain, one might have been tempted to consider General ROY's scale as precisely similar to Mr. RAMSDEN's, and as offering the best source from which the national standard yard might be obtained.

The spirit however, of the recommendation of the Commissioners of Weights and Measures, appearing to be, that the standard yard should be derived *from the base of the*

Trigonometrical Survey, I thought it preferable to proceed a step higher, and to obtain a distance of 40 inches from the iron bar itself, which could afterwards be employed in any manner that might be found most eligible.

In order to obviate the necessity of an allowance for temperature, I caused a triangular bar of cast iron to be made, of the same dimensions as Mr. RAMSDEN's, except as to length. Gold pins were inserted near the extremities of this bar at the distance of 40 inches from each other, on which were to be drawn fine lines, comprising one sixth part of the length of the 20 feet bar.

The apparatus used for tracing the lines on the gold pins, is essentially different from that commonly employed. The cutting point is elevated by means of an inclined plane, and is then carried through a distance equal to the length of the line to be traced. On drawing back a part of the apparatus, the extremity of which acts upon the inclined plane, the point descends by its own weight until it wholly rests upon the surface of the bar; the motion being then continued, the frame and cutting point are drawn along together, without the possibility of lateral deviation; and the point describes a line, the length of which may, by a certain contrivance, be regulated at pleasure, and its strength determined by repeating the operation. This very neat and important invention is due to M. FORTIN of Paris, and was communicated to me by M. ARAGO, whose liberal mind knows no reserve on scientific subjects. I have varied the arrangement of M. FORTIN, so as to bring the cutting point under a microscope furnished with cross wires, having an adjustment, by means of which their intersection can be brought to the line traced by the

cutting point. This, I consider, to be an essential improvement, as no accidental derangement of the cutting frame can take place without its being immediately perceptible; and the apparatus may be conveniently applied to the division of straight lines or circles, in the manner I have described in the Philosophical Transactions for 1814.

The micrometer microscopes, used in the comparison of the different standards, were those employed in the determination of the length of the seconds pendulum, the description of which may be seen in the Philosophical Transactions for 1818. But as the arrangement of Mr. RAMSDEN's bar, required that the support to which the microscopes were attached should rest on its surface, some other form of the beam carrying them became necessary for this purpose.

A board was prepared of well seasoned mahogany, 36 inches long, 9 inches wide, and $\frac{3}{4}$ thick, and an edge bar of mahogany $3\frac{1}{2}$ inches wide and $1\frac{1}{4}$ thick, was firmly fixed along the middle of it lengthwise, which most effectually prevented the possibility of flexure. To the extremities of this edge bar, and projecting beyond them, the microscopes were fixed, their cross wires being about 40 inches asunder. By this arrangement, the very important advantage was ensured, that the apparatus being laid on a plain surface, such as a scale, and the microscopes adjusted to distinct vision, on placing it on another plane scale, the object glasses of the microscopes would be precisely at the same distance from this last surface as they were from that to which they were applied in the first instance, and consequently, no error could arise from parallax.

A piece of very thin brass, usually called latin brass, was

bent round the edges of the 40 inch bar, so that the upper surface of the bar was in perfect contact with the brass, the side pressure being just sufficient to prevent any change of position in the brass, unless when moved along the bar by hand. A fine line, about the eighth of an inch long, was now drawn on one of the gold pins at right angles to the bar, and a similar line was traced on the piece of brass, which was placed so as to cover the other gold pin. The intersection of the cross wires of the tracing microscope was carefully adjusted to this last line.

Mr. RAMSDEN's bar, upon his decease, became the property of Mr. BERGE, whose successor, Mr. WORTHINGTON, kindly granted me access to it, and facilitated my examination by every assistance in his power. The bar was placed in his workshop on tressels, and its surface carefully brought into the same plane, which was ascertained by stretching a thread from end to end.

The 40 inch bar was laid near Mr. RAMSDEN's bar on the 12th of April, 1820, and a thermometer placed upon it. Three thermometers were also arranged at equal distances along Mr. RAMSDEN's bar.

On the 13th of April I commenced my examination. The intersection of the wires of the one microscope being placed on the centre of the left hand dot, the intersection of the wires of the other microscope was brought, by means of its micrometer screw, to the centre of the right hand dot, and the reading of the micrometer registered. In this manner the six intervals of Mr. RAMSDEN's bar were compared in succession. It may be necessary to remark, that as the microscopes invert, the readings are to be taken in a

contrary sense, the higher number indicating defect, and *vice versa*.

	Readings.	Thermometers.
1st interval.	29,5	54,0 } RAMSDEN's bar.
2d.	10,0	53,5 }
3d.	10,0	53,5 }
4th.	16,5	
5th.	10,0	53,0 Forty inch bar.
6th.	19,0	
Mean	15,9	

The difference of temperature of the two bars being so small, may safely be neglected.

The micrometer microscope was now set to 15,9 divisions, and the apparatus being laid on the 40 inch bar, the intersection of the wires of the left hand microscope was brought to the middle of the line on the gold pins, and the piece of latin brass was moved along the bar, till the middle of the line drawn upon it appeared in the intersection of the wires of the micrometer microscope. The whole having been carefully examined, the micrometer microscopes were withdrawn.

The tracing microscope was next brought over the 40 inch bar, and placed so that the intersection of its wires appeared upon the middle of the line traced upon the brass; the brass was then slid away, and a line drawn with the cutting point upon the gold surface.

I had next to compare the distance thus obtained, with the mean of the six intervals on Mr. RAMSDEN's bar.

First comparison.

The four thermometers being at 54°, the following readings were taken.

		Readings.		Div.
Forty inch bar.		33,5		
Ramsden's bar.	1st interval	54,0	Mean of Ramsden's bar	36,5
	2d.	33,0	—— of the forty inch bar	34,2
	3d.	27,0		
	4th.	38,0	Forty inch bar *longer*	2,3
	5th.	30,0		
	6th.	37,0		
	Mean	36,5		
Forty inch bar		35,0		

Second comparison. Thermometers as before.

		Readings.		Div.
Forty inch bar.		36,0		
Ramsden's bar.	1st interval	54,5		
	2d.	31,0	Mean of Ramsden's bar	37,8
	3d.	26,0	—— of forty inch bar	35,5
	4th.	40,0		
	5th.	35,5	Forty inch bar *longer*	2,3
	6th.	40,0		
	Mean	37,8		
Forty inch bar		35,0		

Third comparison. Thermometers as before.

		Readings.		Div.
Forty inch bar		35,0		
RAMSDEN'S bar.	1st interval	58,7		
	2d.	30,0	Mean of RAMSDEN'S bar	39,6
	3d.	31,0	—— of forty inch bar	35,6
	4th.	44,0		
	5th.	34,0	Forty inch bar *longer*	4,0
	6th.	40,0		
	Mean	39,6		
Forty inch bar.		36,2		

By the mean of these comparisons, it appeared that the forty inch bar was *too long* 2,9 divisions of the micrometer, or ,000124 of an inch.*

The preceding measures were taken from the middle of the lines on the gold pins; but as it was found that these lines were not quite parallel, this accidental circumstance afforded a method, of which I availed myself, to attain a greater degree of accuracy.

The deviation of the two lines was obtained by measuring the difference of the distances of their extremities, and by the mean of six comparisons was found to be 16,8 divisions.

Now, as this is the deviation due to the whole length of the lines, they will have approached each other 2,9 divisions, at about one sixth part of their length, reckoning from their most distant extremities.

* Each division of the micrometer is $\frac{1}{23123}$ of an inch

This portion of the line being estimated, transverse lines were drawn, indicating the points from which future measurements were to be taken.

On the 14th of April I resumed my comparisons.

Conceiving that it would be preferable to ascertain the difference between some one interval and the mean of all the intervals of Mr. RAMSDEN's bar, and afterwards to compare such interval with the forty inch bar, I now directed my attention to this object.

Fourth comparison. Thermometers 52°,5.

	Readings.
1st. interval	99,0
2d.	78,0
3d.	73,0
4th.	83,0
5th.	82,0
6th.	83,0
Mean	83,0

15th April. Fifth comparison. Thermometers 56°.

	Readings.
1st. interval	107,0
2d.	81,0
3d.	76,0
4th.	89,0
5th.	77,0
6th.	87,0
Mean	86,1

Sixth comparison. Thermometers 56°.

	Readings.
1st. interval	107,0
2d.	80,0
3d.	79,0
4th.	82,0
5th.	75,0
6th.	83,5
Mean	84,4

On examining the preceding comparisons, it may be perceived that the readings of the sixth interval differ very little from those of the mean of the whole bar.

Readings of the sixth interval.	Mean readings of all the intervals.	Value of the sixth interval + or —.
37,0	36,5	— 0,5
40,0	37,8	— 2,2
40,0	39,6	— 0,4
83,0	83,0	— 0,0
87,0	86,1	— 0,9
83,5	84,4	+ 0,9
	Mean	— 0,5

The sixth interval, therefore, is too short 0,5 of a division.

This interval was now compared with the forty inch bar, the thermometers being at 57°; the microscopes were transferred from one bar to the other alternately.

Readings of the sixth interval.	Readings of the 40 inch bar.
81,5	85,0
85,3	85,0
82,7	86,0
83,0	83,0
83,5	83,0
83,0	82,0
82,5	82,6
82,2	82,6
82,5	81,8
82,0	81,5
82,7	82,5
82,0	81,3
Mean 82,7	83,0

From this it appears, that the forty inch bar is *shorter* than the sixth interval 0,3 of a division ; and as the sixth interval was found to be shorter than the mean of all the intervals 0,5 of a division, the result of the whole is, that the forty inch bar is shorter than one sixth of RAMSDEN's bar 0,8 of a division, or ,000034 of an inch.

I may here remark, that the differences observable between the results of the various comparisons of the intervals of RAMSDEN's bar, may be attributed to the large size and imperfect state of most of the dots; those bounding the sixth interval are fortunately the least injured.

Having thus obtained the value of the standard, from which the chain used in the Trigonometrical Survey was actually laid off, I next proceeded to compare this with General ROY's and Sir GEORGE SHUCKBURGH's scales.

It may be seen in the former part of this paper, that the temperature at which the points were laid off on Mr. RAMSDEN's bar from the brass scale, was 54°; consequently, the observed lengths of the brass scales and 40 inch bar, must be reduced to this temperature. The expansion of one foot of General ROY's scale for one degree of the thermometer was found by him to be ,0001287, and of one foot of RAMSDEN's bar ,0000740 of an inch; consequently, the excess of the expansion of 40 inches of the scale, above 40 inches of the bar, for each degree above 54° will be ,0001637 of an inch; and this quantity has been used in computing the corrections for temperature.

The comparison of both scales with the bar was made at the same time; but to avoid confusion, I have given the results in separate tables. The scales and the 40 inch bar were laid together two days previously to commencing the examination.

TABLE I.

Comparisons of the distance from zero to 40 inches of General ROY's *scale with the 40 inch bar.*

Date.	Temp.	Readings. Bar.	Readings. Roy.	Difference between the scale and the bar in inches	Correction for Temperature.	Roy's scale shorter than the forty inch bar.
May 7	59,0	23	32,5	—,000407	—,000829	,001236
8	62,0	14	23	—,000385	—,001326	,001711
9	65,0	41,5	43,5	—,000086	—,001823	,001909
	65,2	41	40	+,000043	—,001856	,001813
	65,3	30	26	+,000171	—,001873	,001701
	65,3	30	29	+,000043	—,001873	,001829
	64,7	27	25	+,000086	—,001773	,001687
10	67,4	36,2	24	+,000522	—,002220	,001698
					Mean	,001698

By the above comparisons, General Roy's scale appears to be shorter than the 40 inch bar ,001698; to which adding ,000034, the quantity by which Ramsden's bar exceeds the 40 inch bar, we have ,001732 of an inch for the difference *in defect* between General Roy's scale and the standard used in the Trigonometrical Survey, with which it was supposed to be identical.

TABLE II.

Comparisons of the distance from zero to 40 inches of Sir George Shuckburgh's *scale with the 40 inch bar.*

Date.	Temp.	Readings.		Difference between the scale and the bar in inches.	Correction for Temperature.	Shuckburgh's scale *shorter* than the forty inch bar.
		Bar.	Shuck.			
May 7	59.0	23	75,2	—,002234	—,000829	,003063
9	65,0	41,5	62	—,000877	—,001823	,002700
	65,2	41	58	—,000728	—,001856	,002584
	65,3	30	49	—,000813	—,001873	,002686
	65,3	30	51	—,000899	—,001873	,002772
	64,7	27	50	—,000984	—,001773	,002757
10	67,4	36,2	41,5	—,000227	—,002220	,002447
	67,8	33	44	—,000471	—,002287	,002758
12	67,5	42	48	—,000257	—,002237	,002494
					Mean	,002696

If to the above mean ,000034 be added as before, we have ,00273 of an inch, by which the distance from zero to 40 inches of Sir George Shuckburgh's scale is shorter than one sixth part of Ramsden's bar.

The very great difference between Ramsden's bar and General Roy's scale, made me desirous of comparing this last with the Royal Society's standard, and as I was aware of the existence of other standards of considerable importance, I resolved to examine them at the same time.

The Royal Society's scale has been described by Sir George

SHUCKBURGH: it is of brass, and about the same dimensions as General ROY's scale, which is already well known. It has three parallel lines drawn upon it lengthwise. On one of the exterior lines marked E, are two dots expressing the length of the Tower yard. This is the yard which has been heretofore called, and which I shall still call, *the Royal Society's standard*. The middle line has the Exchequer yard marked upon it; and the other exterior line has dots, at precisely the same distance as those of the Royal Society's standard.

Knowing that Mr. CAREY had made for Lieutenant Colonel LAMBTON, a standard scale, which forms the basis of the Trigonometrical Survey carried on by him in India, and aware of the importance of ascertaining the value of this in parts of other known standards, I enquired of Mr. CAREY whence it was derived, and was informed that it had been copied from a scale then in the possession of ALEXANDER AUBERT Esq. and which, after his death, was purchased by Mr. JONES, of Holborn. On application being made by the Commissioners of Weights and Measures to Mr. JONES for the loan of it, their request was readily and obligingly complied with.

This scale is of plate brass, strengthened by an edge bar: it contains 61 inches, and has the name of BIRD upon it. Two dots upon two gold pins designate the yard, from which the divisions of the scale have evidently been derived. There is also a third dot, marking, I believe, the length of the French half toise. The dots indicating the yard are those I employed. I shall call this scale *Colonel* LAMBTON's *standard*.

BIRD's Parliamentary standard yard of 1758, had already been compared with Sir GEORGE SHUCKBURGH's scale by him, and recently by myself, and found to exceed it about two ten-

thousandths of an inch. In Sir GEORGE SHUCKBURGH's "account of experiments for determining a standard of weights and measures," he remarks, that there existed another standard yard made by BIRD, in the year 1760, which did not differ more than two ten-thousandths of an inch from the standard of 1758; but he does not say whether this difference was observed to be in excess or in defect.

As it was possible that this standard yard of 1760, might coincide with 36 inches of Sir GEORGE SHUCKBURGH's scale, I was anxious to compare them together, and by the exertions of DAVIES GILBERT, Esq. M. P. the standard of 1760 was found in the custody of the House of Commons, and confided to my care. It is (as Sir GEORGE SHUCKBURGH observed) precisely similar in form to the standard of 1758, the yard being marked by two dots upon gold pins, which, though very large, are in tolerable preservation.

The five standard scales which I have just described, were placed together on the 15th of June, 1820, and arranged so that those of least bulk should be farthest from me during the observations, as they would be more readily affected by the proximity of the person of the observer.

As I was desirous that comparisons of such importance should not rest wholly on my own authority, I requested Dr. WOLLASTON to take two series of measurements, which, together with my own, are contained in the following table.

TABLE III.

Comparisons of various standards.

Date. 1820.	Temperature.	Readings of the micrometer at: The Royal Society's standard.	General Roy's scale.	Sir George Shuckburgh's scale.	Bird's standard of 1760.	Col. Lambton's standard.
June 16	61,5	19	23	43	43	66
	—	6	17	41,5	41,5	58
	—	18	14	36	36	50
	—	86	92	118	118	133
	64,0	75	93	114	114	121
	—	82	91	111	106	126
	—	78	93	112	108	127
	63,5	75	93	113	110	124
17	61,5	15	24	44	40	59
	—	2	17	39	36	53
	—	1	14	35	36	45
	64,5	59	70	94	94	135
(By Dr. Wollaston) 18	64,5	30	39	64	64	70
(By ditto)	—	21	36	56	56	66
Mean of the whole - - -		40,1	51,1	72,9	71,6	87

I now returned to the forty inch iron bar and General Roy's scale, anxious to verify my former conclusion by a fresh examination. The microscopes being fixed at the proper distance, comparisons were made, which I shall detail before I state the results afforded by the preceding table.

TABLE IV.

Farther comparisons of the distance from zero to forty inches of General ROY*'s scale, with the forty inch bar.*

Date.	Temp.	Readings. Bar.	Readings. Roy.	Difference between the scale and the bar in inches.	Correction for Temperature.	Roy's scale shorter than the forty inch bar.
June 19	64,5	29	30	—,000043	—,001740	,001783
	65,0	28	26	+,000086	—,001823	,001737
	65,5	30	30	—,000000	—,001906	,001906
	66,7	12	5	+,000300	—,002104	,001804
	65,2	93	89	+,000171	—,001856	,001685
20	61,7	0	13	+,000556	—,001276	,001832
	—	2	14	—,000514	—,001276	,001790
	—	0	14	—,000599	—,001276	,001875
	—	1,5	15	—,000577	—,001276	,001853
	—	3	16	—,000556	—,001326	,001882
					Mean	,001815

Adding to the mean thus obtained ,000034, the excess of one sixth of RAMSDEN's bar above the forty inch bar, we have ,001849 of an inch for the excess of 40 inches of the standard used in the Trigonometrical Survey, above General ROY's scale, differing from the result given by the former comparisons contained in Table I. only ,000117 of an inch, a difference which may be attributed to uncertainty of temperature. The mean of both ,00179, is probably very near the truth.

I shall now proceed to give in one view, the results deduced from Table III, by comparing each standard in succession, with that used by Colonel LAMBTON in the survey of India.

Excess of the following standards above Colonel LAMBTON's standard.	On 36 inches.
Sir G. Shuckburgh's standard - - -	+,000642
Bird's standard of 1760, - - - -	+,000659
General Roy's scale - - - -	+,001537
Royal Society's standard - - -	+,002097
Ramsden's bar (used in the Trigonometrical Survey of Great Britain) - - - -	+,003747

If the results of the two series of comparisons made by Dr. WOLLASTON be examined, it will be seen that the greatest difference from those above given, is not two ten-thousandths of an inch, and this difference appears to have arisen almost wholly from the ill defined dots of the Royal Society's standard.

The standard used in the Trigonometrical Survey, being thus unexpectedly found to differ so considerably from every other standard of authority, the Commissioners of Weights and Measures proposed, in their Second Report, that BIRD's Parliamentary standard of 1760, should be considered as the foundation of all legal weights and measures.

It may be seen, that the standard thus selected, differs so little, if at all, from that of Sir GEORGE SHUCKBURGH, that they may, for every purpose, be considered as perfectly identical; and this agreement is particularly convenient, because the length of the mêtre having been determined by comparisons with Sir GEORGE SHUCKBURGH's scale, and a fac simile of it made by Mr. TROUGHTON, for Professor PICTET, all measures on the Continent are converted into English measure, by a reference to Sir GEORGE SHUCKBURGH's standard.

In determining the figure of the earth, by means of the measurement of distant portions of the same meridian, many anomalies have been remarked, which may, in some instances, be attributed to the difference of the standards employed in such measurements. As an example of the importance of this consideration, I shall examine the results deduced by Lieutenant Colonel LAMBTON, from a comparison of three sections of the great arc measured by him in India, with the lengths of the French, the English, and the Swedish degrees. The

abridgement of Lieutenant Colonel LAMBTON's very important operations, may be found in the Philosophical Transactions for 1818.

The following are the data given by Colonel LAMBTON.

The length of the degree due to

Lat.	9°	34′	44″	is 60472,83 fathoms.
	13	2	55	60487,56
	16	34	42	60512,78

By the French measurement, in

Lat.	47	30	46	60779 fathoms

By the English, in

Lat.	52	2	20	60820

By the Swedish, in

Lat.	66	20	12	60955

and by successively comparing the lengths of the European degrees with the three sections of the Indian arc, Colonel LAMBTON obtains for the compression

By the French $\frac{1}{305,73}$ $\frac{1}{306,7}$ $\frac{1}{315,03}$ mean $\frac{1}{309,15}$

By the English $\frac{1}{310,28}$ $\frac{1}{311,36}$ $\frac{1}{318,97}$ mean $\frac{1}{313,54}$

By the Swedish $\frac{1}{305,14}$ $\frac{1}{305,72}$ $\frac{1}{310,72}$ mean $\frac{1}{307,19}$

and the mean of the three means $= \frac{1}{309,96}$.

In order to reduce the preceding measurements to the English national standard, we have to multiply the fathoms of the Indian degree by —,000018, and of the English by +,00007, to obtain the correction to be applied, with its proper sign, to the length of the degree. The French and Swedish degrees require no correction.

We have then the following data for computation.

By sections of the Indian arc	9° 34′ 44″	60471,74	fathoms
	12 2 55	60486,47	
	16 34 42	60511,69	
The French	47 30 46	60779	
English	52 2 20	60824,25	
Swedish	66 20 12	60953	

and the resulting compression,

By the French	$\frac{1}{304,54}$	$\frac{1}{305,55}$	$\frac{1}{313,77}$	mean	$\frac{1}{307,99}$
English	$\frac{1}{305,57}$	$\frac{1}{306,40}$	$\frac{1}{313,50}$	mean	$\frac{1}{308,49}$
Swedish	$\frac{1}{304,44}$	$\frac{1}{305,01}$	$\frac{1}{309,09}$	mean	$\frac{1}{307,55}$

and the mean of the three means $= \frac{1}{307,55}$

As it appears that the compression obtained by employing the length of the degree in Lat. 16° 34′ 42″ is uniformly in defect, whilst the results deduced from the other two sections are very nearly alike, it might perhaps be allowable to consider $\frac{1}{301,33}$, the mean of these last results, as the true compression; and this would agree very nearly with the deduction of M. LAPLACE, from the lunar irregularities; with the result of Dr. YOUNG's interesting and novel investigation, by a comparison of the mean, with the superficial density of the earth; and with the conjecture I have hazarded from the compression given by the experiments on the length of the pendulum at Unst and Portsoy.

3rd August, 1820.

VIII. *An Account of the urinary organs and urine of two species of the genus Rana.* By JOHN DAVY, *M. D. F. R. S.*

Read January 18, 1821.

IN a letter to Sir HUMPHRY DAVY, which was written almost two years ago, and which I understand has been honoured with a place in the Transactions of the Royal Society, I have described the kidneys of many different animals of the class Amphibia, and have shown, with one exception not there insisted on, that their urinary secretion is very similar, consisting almost entirely of uric acid.

The exception which occurred to me, was in the case of frogs, on whose urinary organs and secretion I have had an opportunity, lately, of making fresh and more minute enquiry, the results of which I beg leave to communicate.

I shall first relate the observations I have collected on the urinary organs of these animals, and then describe the experiments I have made to determine the nature of their urine.

The examination I have engaged in, has been limited to two species, the bull-frog (Rana taurina, Cuv.) and the brown-toad (Bufo fuscus, Laurenti) which are both very common in this neighbourhood; the former inhabiting the lake of Colombo, where it occasionally grows to a great size; and the latter frequenting houses, and abounding by night in the streets of the Pettah.

The kidneys of the bull-frog are apart, one on each side of

the spine; comparatively pretty large; very much lobulated; of a bright red colour, and rather tender.

The ureters do not terminate in the bladder, but in the rectum, by two soft papillæ projecting a little, and situated between the orifice of the bladder and the anus, nearer the former than the latter.

The bladder of urine is of large dimensions; nearly globular; semi-transparent, and yet pretty strong and contractile. It opens into the rectum, a few lines behind the anus, by a large orifice, very well adapted to receive the urine, as it flows from the ureters, when the anus is closed, as it usually is, by its powerful sphincter muscle.

The urinary organs of the brown-toad resemble, in most respects, those of the green-frog. In two specimens, out of many that I have dissected, I have found the kidneys incorporated at their upper ends. The ureters have the same termination nearly. The bladder of urine appears to be double; when distended fully with air, it resembles two oval bags; the compartments communicate freely just above the symphysis pubis, to which they are firmly attached; and they have but one orifice into the rectum, which is as well suited as in the former instance, for the reception of the urine as it flows into the rectum.

The urine of the bull-frog, taken from the bladder immediately after the death of the animal, varies a little in its appearance in different instances; and, of course varies considerably in quantity, the bladder being sometimes full almost to distension, and at other times quite empty. The following is a description of a quantity of urine amounting to 300 grains which was collected from thirty-six frogs of different sizes:

It looked like water, and was almost transparent. It was insipid, but not without smell; it emitted an odour not unlike that of serum of blood. It was of sp. grav. 1003.

It is obvious, that with its appearance its chemical nature must also vary. The urine, the physical properties of which I have described, had no effect on litmus or turmeric paper; slowly evaporated, it afforded a minute quantity of brownish extract, which had the smell of urea. It deliquesces when exposed to the air; and when decomposed by heat in a small glass tube, it yielded a little amber-coloured oily fluid and strong ammoniacal fumes; and a coal remained, in which I discovered a large proportion of common salt and a little phosphate of lime.

Another specimen of the urine of these frogs, which I examined, was rather more dilute. I detected in it a minute portion of common salt and of phosphate of lime, without any traces of urea.

The urine of the brown-toad is pretty uniform in different instances in its appearance; and, judging from the experiments I have made, in its nature also. From eighty-four toads, caught in the streets of the Pettah, 732 grains of urine were collected. Examined when quite fresh, it was nearly transparent, and would have been perfectly so, but for a few minute flocculi suspended in it. It was of a pretty bright straw yellow, very like healthy human urine in appearance, with the peculiar smell of human urine, and nearly the same taste in a slight degree. It was of sp. grav. 1008.

It did not alter litmus or turmeric paper. Nitrate of silver dropt into it, produced a very copious precipitate of luna cornea. A solution of corrosive sublimate occasioned a minute

flocculent precipitate. Neutral acetate of lead, a copious white precipitate. *Aqua ammoniæ* had no effect. Oxalate of ammonia produced a slight cloudiness; and a faint cloudiness was produced by muriate of barytes, which did not disappear on the addition of a drop of nitric acid. A portion of this urine, slowly evaporated, afforded a brown extract, with a strong urinous smell. To a moiety of this extract of a syrupy consistence, a drop of nitric acid was added; the effect produced was just the same as if human urine had been the subject of the experiment; a crystalline compound was immediately formed, which I could not hesitate in pronouncing nitrate of urea. The other moiety, decomposed by heat in a close glass tube, afforded a considerable quantity of yellow oily fluid, strongly impregnated with subcarbonate of ammonia, and a residual coal, from which I obtained a large proportion of common salt and a little phosphate of lime, and slight traces of a fixed alkaline phosphate.

Another portion of this urine was set aside to undergo spontaneous decomposition. It has been kept now eight days. It has become slightly turbid, and has acquired a distinct, though not strong ammoniacal odour, mixed with another kind of odour, not unlike that of cabbage.

The conclusions to be drawn from the results of these experiments scarcely need to be pointed out: it is pretty evident, now, that the urine of the bull-frog and of the brown-toad contains urea, and the latter rather abundantly. Reasoning from analogy, the probability is, that the urine of frogs and toads in general is of a similar nature, and altogether different from that of the other amphibia.

It is seldom that any very abrupt transitions are to be

observed in nature: the urinary organs of the turtle and tortoise, seem to be a connecting link between those of the animals in question, and those of serpents and lizards.

Perhaps additional facts are not required to prove, that the secretion of the kidneys of animals depends more on the intimate and invisible structure of these organs, than on the kind of food the animals consume; were such facts wanting, there would be no difficulty in furnishing them. How different is the urine of the brown-toad and that of any species of small lizards! yet flies are the favourite and common diet of both animals. Other remarkable instances might be mentioned, of similarity of diet and difference of urinary secretion; and, *vice versa*, instances might be afforded of difference of diet and similarity of urine: I will mention one only; it is that of parrots and snakes; their urine, as I have found, being much the same, consisting chiefly of uric acid, though their diet is altogether different, the birds feeding entirely on vegetable matter, and the reptiles entirely on animal matter. But let me not be supposed to maintain that the urinary secretion depends entirely on the organ, quite independent of the nature of the food or of the blood, from which the elements of the urine are derived. It appears to be pretty satisfactorily proved, that, *cæteris paribus*, there is a certain relation between the nature of the food and of the urine. Whilst this has been generally admitted, the relation between the organ and the secretion has been less insisted on, though perhaps not less curious and deserving of attention.

I have hitherto made no allusion to the difference of opinion amongst comparative anatomists, on the subject of the urinary organs of the frog, whether it has, or has not, an urinary

bladder; nor do I propose now, more than barely to allude to it. I flatter myself, that the analysis I have given of the contents of the bladder of the frog and toad, not to mention the anatomical observations detailed, will satisfy the sceptical, that the bladder of these animals is really a bladder of urine, according to the opinion long since advanced by M. CUVIER.

Colombo, Ceylon, January 28, 1819.

IX. *An account of a Micrometer made of Rock Crystal.*

By G. Dollond, *F. R. S.*

Read January 25, 1821.

Rock crystal having been applied to telescopes in various ways, for the purposes of micrometrical measurements, particularly that which is recommended by M. Arago, induced me to consider if a more simple mode of applying the crystal could not be discovered; and the following account of its application to the eye tube of a telescope, is the result.

The improvement consists in making a sphere or lens from a piece of rock crystal, and adapting it to a telescope in the place of the usual eye-glass; and from its natural double refracting property, rendering it useful as a micrometer.

The advantages of thus applying the crystal are, in the first place, the very great saving of the time required to find the proper angle for cutting the crystal; also of cutting the prisms to their proper angles, and working their surfaces with sufficient accuracy to render them useful as micrometers, in the manner that is recommended by M. Arago, Dr. Wollaston, and others.

Upon the plan which is now submitted, it is only necessary to select a piece of perfect crystal; and without any knowledge of the angle that will give the greatest double refraction, to form the sphere of a proper diameter for the focal length required.

The second advantage is derived from being able to take the angle on each side zero, without reversing the eye tube;

also of taking intermediate angles between zero and the greatest separation of the images, without exchanging any part of the eye tube, it being only required to move the axis in which the sphere is placed.

Thirdly, it possesses the property of an eye tube or lens that is not intended for micrometrical measurements; for when the axis of the crystal is parallel to the axis of the object-glass of the telescope, only one image will be formed, and that will be as distinctly formed as with any lens that does not possess the double refracting property.

The eye tube is so constructed, that the plane through which the two images move, can be placed parallel to the line in the object which is to be measured; and if this motion is furnished with a divided circle, it will correctly answer the purpose of a position micrometer.

The value of the scale is found from the known diameter of any distant object, and will vary in proportion to the magnifying powers of the eye tube; its value increasing in proportion to the increase of those magnifying powers.

The preceding remarks appearing to be sufficient to elucidate the novelty of the application, I shall now endeavour to render the contrivance more explicit by references to the plate. [See Pl. IX.]

Fig. 1. Is a section of the eye tube; and Fig. 2. a general view of the same; both of the full size.

The sphere or lens, *a*, fig. 1. is formed of rock crystal, and placed in half holes, from which is extended the axis *b*, *b*, with an index attached; which index registers the motion of the sphere, the extent of that motion being shown upon the divided face *c*, fig. 2. The sphere is so placed in the half

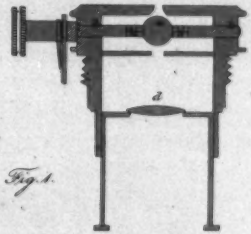


Fig. 1.

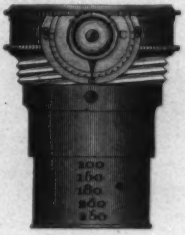


Fig. 2.

holes, that when its natural axis is parallel to the axis of the telescope, only one image of the object is seen. In the other direction, or that which is at right angles to the axis of motion, it must be so placed, that when it is moved, the separation of the images, viz. the ordinary and extraordinary, may be parallel to that motion. The method of acquiring this adjustment is, by turning the sphere in the half holes parallel to its own axis.

The field of view of the eye tube is increased, and the magnifying power varied, by the introduction of the lens *d*, fig. 1, between the sphere and the primary image of the object-glass; and its distance from the sphere will be in proportion to the magnifying power required; the magnifying powers are engraved upon the eye tube at *e*, fig. 2, and will vary in proportion to the focal length of the object-glass to which the eye tube is applied.

Those marked in the figure, are for an object-glass of 44 inches in focal length.

When I constructed this micrometer, it was my intention to have applied it to the measurement of the angles that are subtended by the apparent diameters of the fixed stars, as seen in achromatic refracting telescopes, for the purpose of determining their relative magnitudes; also of measuring the distances of those double stars that would come within the range of the micrometer; but from being compelled to attend to business of more immediate consequence, I am not able to accompany this description with any measurements that are sufficiently important to be interesting; although I am fully convinced from the trials I have made, that the micrometer is quite equal to the purposes for which it was intended.

X. *The Bakerian Lecture. On the best kind of steel and form for a compass needle. By Capt.* HENRY KATER, *F. R. S.*

Read February 1, 1821.

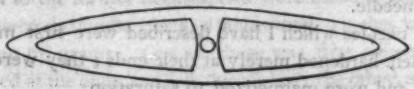
ON the return of the first expedition which sailed for the discovery of a north-west passage, it appeared that from the near approach to the magnetic pole, and the consequent diminution of the directive force, the compasses on board had become nearly useless. Some of the azimuth compasses employed on that occasion were of my own invention; I was therefore anxious that the next expedition, which was about to sail under the command of Lieutenant PARRY, and which has happily returned with so much honour to those engaged in it, should be furnished with instruments of this description, combining as much power and sensibility as possible.

It was with this intention alone that I commenced the experiments which form the subject of the present paper; but which I should not have deemed sufficiently important to be made public, had I not lately, on resuming the enquiry, been led to some results which appeared of sufficient interest, as well as practical utility, to induce me to lay them before the Royal Society.

My immediate object was to ascertain the kind of steel, and form of needle best calculated to receive the greatest directive energy with the least weight.

Two needles were prepared of that kind of steel which is called blister steel, and two of spur steel, the weight of each

being 66 grains. They were of the form of a long ellipse, in length 5 inches, and in width half an inch. One of each kind was pierced, as in the figure below, the weight being made up by additional thickness. This needle, therefore, had much less extent of surface than the solid ellipse.



Recollecting to have had in my possession, many years since, a compass of extraordinary power, the needle of which was composed of pieces of steel wire put together in the shape of a rhombus, I caused two needles to be made of this form of a piece of clock-spring, which I understand is of that kind of steel which is called shear steel. They were shaped as below; in one the cross piece was of brass, and in the other formed of part of the clock spring. These needles were, by mistake, made to weigh only 45 grains.

In ascertaining the directive force, the balance of torsion of M. Coulomb was employed. This instrument, as is well known, consists of a fine wire attached to an index moveable round a circle, divided into degrees. To the other end of the wire is fixed a cradle, to receive the needle which is the sub-

ject of experiment. The needle being in the magnetic meridian when the wire has no torsion, is afterwards forced to deviate from it to a mark distant about 60°, by turning the index, and consequently twisting the wire. The number of degrees passed over by the index will be as the directive force of the needle.

The needles which I have described were first made soft, and then hardened merely at their ends; they were not polished, and were magnetized to saturation.

Experiment 1.

Needles soft, and then hardened at the ends.	Weight of needle.	Directive force.
Blister steel, solid ellipse	66	500
———, open ellipse	66	520
Spur steel, solid ellipse	66	540
———, open ellipse	66	570
Shear steel, rhombus	45	435
———, rhombus, with cross piece of brass	45	435

By the experiments on magnetism made by M. COULOMB, it appears, that the directive forces of needles of similar form are to each other as their masses; the directive force, therefore, of a needle of the form of a pierced rhombus of 66 grains, would be expressed, according to the preceding experiments, by 638.

From many other experiments, which I regret were not registered at the time, it appeared that shear steel was capable of receiving the greater magnetic force, and that the

pierced rhombus was the best form for a compass needle. I may add, that needles of cast steel were tried, but were found so very inferior as to be at once rejected.

My next object was to determine the effect of polish, and of various modes of hardening and tempering the needles. In addition to the former needles, two were made of *clock-spring* of the pierced rhombus form, 5 inches long, 2 inches wide, and weighing 66 grains. One of these was first softened, then hardened at the ends, and left unpolished; the other, as well as the solid elliptical needle of spur steel, was hardened throughout, and polished. The needles were then magnetized to saturation.

Experiment 2.

	Directive force.
Unpolished rhombus, hard at the ends	800
Polished rhombus, hard throughout -	367
Polished elliptical needle, hard throughout	380
Polished elliptical needle, softened in the middle by laying it on a red hot poker	610
Polished rhombus, softened in the middle in the same manner	590
The needles were now laid aside till the following day, when the directive force was again examined.	
Unpolished rhombus, hard at the ends -	805
Polished elliptical needle, softened in the middle - - -	625
Polished rhombus, softened in the middle	580

The polished rhombus was now softened throughout, and the extremities being hardened at a red heat, the directive force was found to be 800. It is scarcely necessary to say, that the needles were re-magnetized to saturation previous to each experiment.

From these experiments I drew the following conclusions.

That of the steel I employed, shear steel is the best kind for compass needles.

That the best form for a compass needle is that of a pierced rhombus.

That polish has no influence on the directive force.

That hardening the needle throughout, considerably diminishes its capacity for magnetism.

That a needle soft in the middle, and its extremities hardened at a red heat, appears to be susceptible of the greatest directive force.

That the directive force does not depend on the extent of surface, but on the mass.

I might also have inferred, that the needle was capable of a greater directive force when wholly softened and hardened at the extremities, than when entirely hardened and softened in the middle; but it will appear by subsequent experiments, to be detailed, that the difference is probably to be attributed to a difference in the degree of heat to which the needle is exposed in softening it in the middle.

My next experiments were made with three needles, two of which were rectangular parallelograms of equal length and weight, but the one only half the width of the other. The third needle was a pierced rhombus; the whole were made of

clock spring. These needles were made perfectly hard, and magnetized, as was always the case, to saturation.

Experiment 8.

Needles perfectly hard.	Directive force.
Wide parallelogram -	490
Narrow parallelogram	490
Pierced rhombus - -	532

An accident happened to these needles, which rendered them unfit for farther experiment. It however appears from that above stated, that the directive force is nearly as the mass, and not as the surface; and that the pierced rhombus is superior to the parallelogram.

M. Coulomb having found that a needle of the rhombus form not pierced, and which he calls "une lame taillée en flèche", was susceptible of a greater directive force than a parallelogram, I was desirous of repeating this experiment, as well as of comparing this form with the pierced rhombus. For this purpose four needles were made four inches and a half long, each weighing 68 grains; one was a parallelogram, 0,44 inch wide; another a rhombus, which I shall call the large rhombus, 0,8 wide; the third a pierced rhombus, 1,4 wide in the middle, having its sides 0,2 wide: these were made of clock-spring. The fourth needle a rhombus, which may be called the small rhombus, 0,4 wide, was made of that kind of steel which is used for saw blades, and which I believe is shear steel. This last needle was much thicker than the others.

The steel of which these needles were made had been exposed to a sufficient degree of heat to render it soft enough to be worked, and in this state the needles were magnetized.

Experiment 4.

Steel soft as worked.	Directive force.
Parallelogram - -	720
Small rhombus -	580
Large rhombus - -	765
Pierced rhombus - -	818

Experiment 5.

The ends of the needles hardened at an obscure red heat.	Directive force.
Parallelogram - -	715
Small rhombus - -	577
Large rhombus -	790
Pierced rhombus - -	840

Experiment 6.

The ends hardened at a red heat.	Directive force.
Parallelogram - -	742
Small rhombus - -	583
Large rhombus -	745
Pierced rhombus - -	844

Experiment 7.

Hardened with a bright red heat, and then softened by a red heat from the middle towards the ends, the extremities for about an inch remaining hard.	Directive force.
Parallelogram - -	611
Small rhombus - -	710
Large rhombus - -	660
Pierced rhombus - -	685

Experiment 8.

Softened at a red heat between two plates of steel, the whole being allowed to cool gradually, and then the extremities of the needles hardened at a red heat.	Directive force.
Parallelogram - -	520
Small rhombus - -	585
Large rhombus - -	534
Pierced rhombus - -	590

As it appeared from the above experiments that the needles had suffered a gradual deterioration, I imagined that this might have occurred in consequence of their having been exposed to the heat of a coal fire, by which some portion of the carbon of the steel might have been destroyed; I therefore re-carbonized the needles, by surrounding them with shreds of leather, and exposing them for several hours in a close vessel to a considerable heat. After they had gradually cooled, the ends were hardened at a red heat.

Experiment 9.

Needles soft, and then the ends hardened at a red heat.	Directive force.
Small rhombus - -	477
Large rhombus - -	415
Pierced rhombus -	450

Here we may remark, that though the needles were apparently in the same state, except being re-carbonized, as in the last experiment, they had suffered considerable deterioration.

The needles were now covered with a mixture well known to workmen to prevent decarbonization: this had been before neglected, but was used in all the subsequent experiments. They were hardened at a bright red heat, and afterwards tempered throughout rather beyond a blue colour. The large rhombus and the parallelogram were accidentally broken.

Experiment 10

Hardened at a bright red, and then tempered beyond a blue.	Directive force.
Small rhombus - -	660
Pierced rhombus -	577

From these last experiments I believe little can be gathered, except that the needles became less susceptible of directive force from repeated exposure to heat, and that this effect was not occasioned by a decarbonization of the steel. The small rhombus of saw blade, perhaps, from being the thickest, suffered less than those made of clock-spring.

The springs of clocks are made by passing the steel between rollers; and it thus undergoes great compression. May not this state be favourable to magnetism; and the repeated expansion of the steel by heat, destroying this state, have occasioned the deterioration I have remarked?

The needle which was made of saw blade having suffered less than the others in the preceding experiments, I procured three other needles of this material; they were cut out of the same plate; the weight of each was 120 grains, and their length four inches and a half. One was a parallelogram, 0,46 inch wide; another a rhombus, as before, 0,87 inch wide; and the third a pierced rhombus, having the middle 1,5 inch, and its sides 0,25 wide.

These needles were made without its being found necessary to soften the steel plate; they consequently were all as nearly as possible of the same degree of temper. In this state they were magnetized.

Experiment 11.

Steel the same as worked.	Directive force.
Parallelogram	1148
Rhombus - -	1090
Pierced rhombus -	1085

Wishing to try whether the needles were magnetized to saturation, I carefully re-magnetized them.

Experiment 12.

Needles re-magnetized.	Directive force.
Parallelogram - -	1140
Rhombus - -	955
Pierced rhombus -	1069

I now to my surprise found that the directive force, instead of increasing, had lessened in each of the needles, and I became anxious to discover the cause of so unexpected a phenomenon. It has been observed by M. COULOMB, and more fully entered into by BIOT, that if a needle be magnetized to saturation by strong magnets, and afterwards weaker magnets be applied, the needle will lose some part of the force it had before acquired. Now, if in using the same set of magnets a certain degree of force be communicated to a needle, and the magnets be afterwards arranged in a manner less favourable for imparting magnetic force, it should seem, that this second operation would produce the same effect as would follow the use of magnets of less force, and that the magnetism of the needle would suffer a diminution.

The method I had employed in magnetizing the needles, was that of DU HAMEL, by joining the opposite poles of the magnets, and placing them on the centre of the needle, so inclined that each formed an angle, as I afterwards ascertained, of about 30 degrees with the horizon. The magnets were then slid from the centre to the extremities of the needle, and their poles being again joined at a distance from the needle, the operation was repeated.

As I could in no way account for the diminution of directive

force which I have remarked, except by supposing that I had inadvertently changed the inclination at which the magnets were held, I resolved to try whether a variation of this angle produced any considerable difference in the degree of magnetism communicated. For this purpose I re-magnetized the needles, by laying the magnets, with their opposite poles joined, flat upon the needle, the junction of the magnets being upon the centre. They were then separated and drawn to the extremities of the needle, the surface of the needle and that of the magnets being in contact the whole time. The poles were then joined and the operation repeated, using but little pressure.

Experiment 18.

Magnets moved flat upon the needle with little pressure.	Directive force.
Parallelogram - - -	1265
Rhombus - - -	1048
Pierced rhombus -	1180

This manner of magnetizing, therefore, appears much superior to that before employed.

The needles were again magnetized in the same manner as in the last experiment, except that the ends of the magnets were pressed pretty strongly against the needle.

Experiment 14.

Magnets moved as before, but with strong pressure.	Directive force.
Parallelogram -	1268
Rhombus -	1005
Pierced rhombus -	1181

No advantage appears to have resulted from the increased pressure, but the arrow-shaped needle has suffered a diminution of power

The magnets were now slid from the middle to the extremities of the needle at an inclination of only two or three degrees, and the following were the results.

Experiment 15.

Magnets inclined in an angle of two or three degrees.	Directive force.
Parallelogram - -	1275
Rhombus - -	1051
Pierced rhombus - -	1156

This method appears to be preferable to any I have yet tried, and was therefore employed in the subsequent experiments of the present series. The rhombus of 63 grains, which in Experiment 10 was left with a directive force of 577, on being magnetized in this manner had its power increased to 600.

The ends of the needles were now hardened at a red heat, the middle remaining soft, as before.

Experiment 16.

Needles hardened at the ends at a red heat.	Directive force.
Parallelogram - -	1315
Rhombus - -	1080
Pierced rhombus - -	1185

Experiment 17.

Ends hardened at a bright red.	Directive force.
Parallelogram - -	1258
Rhombus - -	970
Pierced rhombus - -	1085

The ends hardened at a red heat as near to that employed in Experiment 16, as possible.

Experiment 18.

Ends hardened at a red heat.	Directive force.
Parallelogram -	1350
Rhombus - -	1121
Pierced rhombus -	1205

Here it should seem that an increase of power has been

obtained by the ends of the needles having been first hardened at a higher temperature, and then at a lower.

The needles were now hardened throughout at a bright red heat.

Experiment 19.

Hardened throughout at a bright red.	Directive force.
Parallelogram - -	1120
Rhombus - -	1205
Pierced rhombus - -	1080

The needles softened by laying them on a red hot poker till they passed beyond the blue to a greyish white. This was carried to within an inch of their extremities, which remained hard.

Experiment 20.

Softened from the middle to a greyish white, ends hard.	Directive force.
Parallelogram - -	1860
Rhombus - -	1140
Pierced rhombus -	1210

The tempering was carried throughout the needles, the parallelogram was reserved for another purpose.

Experiment 21.

Softened throughout to a greyish white.	Directive force.
Rhombus - -	1075
Pierced rhombus -	1145

Experiment 22.

Softened throughout to a greyish white, the ends hardened at a red heat.	Directive force.
Rhombus - -	1025
Pierced rhombus -	1185

Experiment 23.

Hardened throughout, and then softened to a greyish white, as in Experiment 21.	Directive force.
Rhombus - -	1065
Pierced rhombus -	1180

This last series of experiments presents a curious circumstance. From the experiments made by COULOMB, as well as from the general tenor of my own, the rhombus is found capable of receiving a greater directive energy than the parallelogram; yet here we perceive that the parallelogram, though formed of the very same plate of steel as the other needles, is not only under every circumstance superior to the rhombus, but also to the pierced rhombus. It is difficult to form any plausible conjecture as to the cause of this difference.

The weight of the rhombus in Experiment 10, made of clock spring, was 63 grains; that made of saw blade weighed 190 grains, or very nearly double. The directive energy of the former, after having suffered great deterioration, and when not tempered in the most favourable manner, compared with the greatest directive energy of the latter, was as 600 to

1210; but if we refer to Experiment 6, it may be seen that the greatest directive energy of the clock spring rhombus was 844, which gives it an advantage of about one third in directive energy over a needle of equal weight made of saw blade.

From Experiment 20, it should seem that a needle is susceptible of the greatest directive power, other circumstances being similar, when it is hardened throughout at a red heat, and then softened from the middle to within an inch of the extremities, till the blue colour which arises has again disappeared.

I next proceeded to try, in a more regular manner, the effect of different methods of magnetizing, and at the same time to ascertain whether the directive force was influenced by extent of surface, independent of mass. Two needles were made of the same kind of steel, in the form of right-angled parallelograms, five inches long, the one 0,7 inch wide, and the other half this width. The widest was reduced in thickness until it was of the same weight as the other, viz. 142 grains. They were in the same state of softness as was necessary to work them. The magnets were placed together perpendicularly on the centre of the needle, their opposite poles being joined; their lower extremities were then separated and kept asunder by placing a piece of wood a quarter of an inch thick between them, their upper extremities remaining in contact. The magnets were then slid along the needle backwards and forwards from end to end: this was repeated on both sides, till it was conceived the needle must be saturated.

Experiment 24.

	Directive force.
Small parallelogram -	655
Large parallelogram -	674

The needles were again magnetized in the same manner as before, excepting that the magnets were separated at the top by a piece of wood of the same thickness as that at the bottom.

Experiment 25.

	Directive force.
Small parallelogram -	595
Large parallelogram -	580

The magnets were placed perpendicularly together on the centre of the needle, and then their lower extremities separated by a piece of wood to the distance of half the length of the needle, the upper extremities remaining in contact. They were then slid on the needle backwards and forwards from end to end, as before.

Experiment 26.

	Directive force.
Small parallelogram -	760
Large parallelogram -	780

The magnets joined, placed perpendicularly on the centre of the needle, as before, then moved in opposite directions from the centre to the extremities, keeping each magnet perpendicular to the needle; afterwards joined at a distance from the needle, placed again on its centre, and the operation thus continued.

Experiment 27.

	Directive force.
Small parallelogram -	993
Large parallelogram -	1155

Remarking that the surface of the small parallelogram was unequal, so as to be touched by the magnet in very few places, I filed it flat, and having reduced the large parallelogram to the same weight, they were magnetized by joining the magnets, placing them perpendicularly on the centre of the needle, separating their lower extremities, and carrying them to each end of the needle, the upper ends remaining in contact.

Experiment 28.

	Directive force.
Small parallelogram -	1025
Large parallelogram -	1150

The needles were next magnetized according to the method of DU HAMEL, the magnets being inclined at an angle

of about 45 degrees, and carried, as before, from the centre to the ends of the needle.

Experiment 29.

	Directive force.
Small parallelogram -	1070
Large parallelogram -	1170

The magnets forming with the needle an angle of about 20 degrees.

Experiment 30.

	Directive force.
Small parallelogram -	1085
Large parallelogram -	1195

Magnets forming an angle with the needle of about two or three degrees.

Experiment 31.

	Directive force.
Small parallelogram -	1160
Large parallelogram -	1275

Magnets laid flat on the surface of the needle, and drawn from the centre to the ends.

Experiment 32.

	Directive force.
Small parallelogram -	1158
Large parallelogram -	1261

Magnets forming with the needle an angle of two or three degrees, their other extremities being connected by a very soft iron wire.

Experiment 33.

	Directive force.
Small parallelogram -	1145
Large parallelogram -	1261

The iron wire was now removed and the needle magnetized, as before, at an angle of about two or three degrees.

Experiment 34.

	Directive force.
Small parallelogram -	1160
Large parallelogram -	1273

I now hardened both the needles throughout at a bright red, and then softened them from the middle to within three quarters of an inch of the ends till the blue had disappeared. This was done by laying the large parallelogram on a red

hot poker, but from the thickness of the small parallelogram this heat was found insufficient, and that of a lamp was employed. The needles were then magnetized as in the last experiment.

Experiment 35.

	Directive force.
Small parallelogram -	1815
Large parallelogram -	1660

It occurred to me that the heat employed in tempering the large parallelogram might not have been sufficient, it was therefore exposed to the flame of the lamp, but in doing this, a small piece which weighed 10 grains was broken off from its end. It was however re-magnetized, and the directive force was now found to be increased to 1720.

From these last experiments, it appears that the greatest directive force was given to the needle when the magnets were inclined to it in an angle not exceeding two or three degrees, and that this force is little, if at all, influenced by extent of surface; as I conceive the small difference in favour of the greater surface may be attributed to some difference in the quality of the steel, or its temper, both of which appear to have very considerable influence on the directive force.

Two needles, the one five, and the other eight inches long, were cut out of the same plate of steel; they were of equal weight, the short one being of greater width than the other. Being magnetized to saturation, their directive forces were as follow:

Experiment 36.

	Directive force.
Long parallelogram -	2275
Short parallelogram -	1193

They were now hardened at a red heat, and tempered beyond the blue from the middle to within an inch of the extremities.

Experiment 37.

	Directive force.
Long parallelogram -	2277
Short parallelogram -	1865

If the mean of these two experiments be taken, it will be found, as was observed by COULOMB, that the directive force of a needle of a greater length than 5 inches is probably as its length.

My next object was to repeat the very interesting experiments recently published by Mr. BARLOW, proving the attraction of iron on a ship's compass to be dependant wholly on extent of surface. For this purpose I had three cylinders made of soft iron, about two inches and a half in diameter, and nearly the same in height. One of the cylinders was of sheet iron, less than the 20th of an inch in thickness; the second of that kind called chest plate, 0,185 inch thick; and the third was of solid wrought iron. The first weighed 2760, the second 9376, and the solid cylinder 22929 grains. Pre-

vious to the experiments they were all made red hot, to destroy any accidental magnetism.

The compass employed was of a very delicate construction, and the cylinder was so placed that its centre was in the direction of a tangent to the zero of the compass, and at the distance of 4,85 from the southern extremity of the needle. The position of the cylinder was varied six times, and the following were the deviations of the needle.

	Sheet iron cylinder.	Chest plate cylinder.	Solid cylinder.
	° ′	° ′	° ′
	2.15	2.50	2.55
	2.15	3. 4	3.15
	2.45	3.20	2.57
	2. 5	3.45	2.50
	2. 5	3.10	2.55
	2.10	3.30	2.30
Mean	2.16	3.16	2.54

Suspecting an error in the experiments with the solid cylinder from an accident which occurred, I repeated the whole with the utmost attention. The position of each cylinder was now varied eight times.

	Sheet iron cylinder.	Chest plate cylinder.	Solid cylinder.
	° ′	° ′	° ′
	2. 3	2.55	3.15
	2.22	2.50	3.12
	2.32	3.20	3.15
	2.20	3.40	3. 0
	1.50	3.40	3.15
	2.45	3.28	2.50
	2.45	3.10	2.45
	1.55	3. 5	2.58
Mean	2.19	3.16	3. 4

The surfaces of the cylinders determined by very careful measurement were, the sheet iron, 28,54; the chest plate, 30,77; and the solid cylinder, 28,94 inches.

Reducing the deviations to the same extent of surface, viz. that of the solid cylinder, they become respectively 141, 184, and 184 minutes.

These last results perfectly coincide with the deductions of Mr. BARLOW, that the effect of iron on a ship's compass is as the surface, and is wholly independent of the mass; but that a certain degree of thickness of the iron (about two tenths of an inch) is necessary to the complete developement of this effect.

The following are the principal inferences which may be drawn from the experiments I have detailed.

That the best material for compass needles is *clock spring*; but care must be taken in forming the needle to expose it as seldom as possible to heat, otherwise its capability of receiving magnetism will be much diminished.

That the best form for a compass needle is the *pierced rhombus*, in the proportion of about five inches in length to two inches in width, this form being susceptible of the greatest directive force.

That the best mode of tempering a compass needle is, first to harden it at a red heat, and then to soften it from the middle to about an inch from each extremity, by exposing it to a heat sufficient to cause the blue colour which arises again to disappear.

That in the same plate of steel of the size of a few square inches only, portions are found varying considerably in their

capability of receiving magnetism, though not apparently differing in any other respect.

That polishing the needle has no effect on its magnetism.

That the best mode of communicating magnetism to a needle, appears to be by placing it in the magnetic meridian, joining the opposite poles of a pair of bar magnets (the magnets being in the same line), and laying the magnets so joined flat upon the needle with their poles upon its centre; then having elevated the distant extremities of the magnets, so that they may form an angle of about two or three degrees with the needle, they are to be drawn from the centre of the needle to the extremities, carefully preserving the same inclination, and having joined the poles of the magnets at a distance from the needle, the operation is to be repeated ten or twelve times on each surface.

That in needles from 5 to 8 inches in length, their weights being equal, the directive forces are nearly as the lengths.

That the directive force does not depend upon extent of surface, but in needles of nearly the same length and form, is as the mass.

That the deviation of a compass needle occasioned by the attraction of soft iron, depends, as Mr. Barlow has advanced, on extent of surface, and is wholly independent of the mass, except a certain thickness of the iron, amounting to about two tenths of an inch, which is requisite for the complete developement of its attractive energy.

XI. *Notice respecting a volcanic appearance in the Moon, in a Letter addressed to the President. By Captain* HENRY KATER *F. R. S.*

Read February 8, 1821.

DEAR SIR,

London, February 8th, 1821.

IT may perhaps be interesting to the Royal Society to be informed, that on Sunday evening, the 4th instant, I observed a luminous spot in the dark part of the moon, which I was inclined to ascribe to the eruption of a volcano.

The telescope used was an excellent Newtonian of $6\frac{1}{4}$ inches aperture, with a power of 74. The moon was exactly two days old, and the evening so clear, that I was able to discern the general outlines in the dark part of her disk. Her western azimuth was about 70°, and her altitude about 10 degrees.

In this position at 6 hours 30 minutes, the volcano was situated (estimating by the eye) as in the accompanying sketch. [See Plate X.] Its appearance was that of a small nebula subtending an angle of about 3 or 4 seconds.

Its brightness was very variable; a luminous point, like a small star of the 6th or 7th magnitude, would suddenly appear in its centre, and as suddenly disappear, and these changes would sometimes take place in the course of a few seconds.

On the evening of the 5th, having an engagement which prevented my observing it myself, I arranged the telescope

for two friends, who remarked the same phenomena as the night before, but in an inferior degree, partly perhaps in consequence of the evening not being so favourable.

On the 6th I again observed it; it had certainly become more faint, and the star-like appearance less frequent. I could see it very distinctly with a power of 40. As the moon approached the horizon, it was visible only at intervals when the star-like appearance took place. On the same evening I had the pleasure of showing it to Mr. HENRY BROWNE, F. R. S.

I regret that I had no micrometer adapted to my telescope; but I have reason to believe the distance of the volcano from the edge of the moon was about one tenth of her diameter, and the angle it formed this evening with a line joining the cusps was about 50°.

I remarked near the edge of the moon, a well known dark spot, from which the volcano was distant, as nearly as I could estimate, three times its distance from the edge of the moon.

In a map of the moon published by Dr. KITCHENER (and which is the best small map with which I am acquainted), there is a mountain sufficiently near the situation of the volcano, to authorize the supposition that they may be identical.

On the 7th I could still see the volcano, and the occasional star-like appearance; but I do not think it was sufficiently perceptible to have been discovered by a person ignorant of its precise situation. I am inclined however to think, that the difficulty of seeing it is rather to be attributed to the

increased light of the moon, than to the diminished action of the volcano.

I have the honour to be,

Dear Sir, &c. &c.

HENRY KATER

To Sir Humphry Davy, Bart.
P. R. S. &c.

P. S. Since the preceding letter was written, I have ascertained that the spot in which I observed the volcanic appearance is that named ARISTARCHUS. This spot was particularly examined by HEVELIUS, who calls it Mons Porphyrites, and who considers it to be volcanic. If his drawings are to be relied upon, it has undergone a considerable change in its appearance since his time.

Sir WILLIAM HERSCHEL has recorded in the Philosophical Transactions an observation of three volcanoes, which he perceived in the moon, April 19th, 1787, at $10^h.36'$, sidereal time. One of these, which he says showed "an actual eruption of fire or luminous matter," was distant from the northern limb of the moon $3'.57''.8$, the diameter of the burning part being not less than $3''$. I find that this observation was made about 9 o'clock in the evening, when the moon was not quite two days old; and from the situation of the spot described by Sir WILLIAM HERSCHEL, I have no doubt of its being the same that I have noticed.

XII. *A farther account of fossil bones discovered in caverns inclosed in the lime stone rocks at Plymouth. By* Joseph Whidbey, *Esq. In a Letter addressed to Sir* Everard Home, *Bart. V. P. R. S.*

Read February 8, 1821.

Borisand Lodge, near Plymouth,
11th Nov. 1820.

Dear Sir,

In November 1816, I sent to Sir Joseph Banks, some fossil bones found in the lime stone quarries at Oreston, near Plymouth, which bones were submitted by him to your examination; and as you considered them to be of some importance, a description of them was laid before the Royal Society, on the 27th of February, 1817.

I now take the liberty of sending you some more bones that have been subsequently found, nearly similarly situated, and not far from the place where the others were discovered, and I beg you will please to make use of these in any way you may think proper for the benefit of science.

These bones were lately found in a cavern one foot high, eighteen feet wide, and twenty feet long, lying on a thin bed of dry clay at the bottom; the cavern was entirely surrounded by compact lime stone rock, about eight feet above high water mark, fifty-five feet below the surface of the rock, one hundred and seventy-four yards from the original face of the quarries, and about one hundred and twenty

yards, in that direction, from the spot where the former bones were found in 1816.

All this quarry had been worked by blasting through the solid rock, with here and there a few small caverns similar to that where the bones were discovered, but none of them had the smallest appearance of ever having had any opening to the surface, or connection with it whatever, or with each other. The caverns here spoken of were quarried many feet below the bottom of them, and nothing was found but hard solid lime-stone, in which the quarrying ceased, and the workmen proceeded on in an horizontal direction.

Many caverns have been met with in these quarries, the insides of which have been crusted with stalactite; but there was no appearance of this kind in the cavern where the bones were found, every part of it being perfectly dry, and nearly clear of rubbish, a circumstance which clearly proves it had no connection with the surface, as in that case water would have found its way into it, the dropping of which would have formed stalactite, as in other instances.

I also send you some other bones, found about the same time, four yards distant from those just mentioned, and I have reason to believe on the same level, and under exactly similar circumstances.

To this account Sir EVERARD HOME has added the following description of the bones, and the names of the animals to which they appear to have belonged.

1. The fourth grinder from the front, on the right side of the upper jaw, of the single horned rhinoceros.
2. The hindermost grinder but one, on the left side of the lower jaw, of the black or brown bear.
3. The hindermost grinder but one, on the right side of the upper jaw, of the black or brown bear.
4. Tusk of the left side of the lower jaw.

   Tusk of the left side of the upper jaw of the brown or black bear.
5. Portions of two tibiæ, apparently of the same animal.
6. One lumbar? vertebra.

   Portion of the os innominatum?

   Portion of the sacrum.

   Head of the os femoris.

   Two portions of cannon? bone of an animal of the deer kind.

   Portions of two dorsal vertebræ.

   Small portion of the pelvis, with part of the acetabulum.

   Part of the ulna.

   Part of the body of the os femoris.

All apparently of the same animal, which is of the size of a bear.

The bones are deposited in the Museum of the Royal College of Surgeons.

XIII. *On the aëriform compounds of Charcoal and Hydrogen; with an account of some additional experiments on the gases from oil and from coal.* By WILLIAM HENRY, M. D. F. R. S. &c.

Read February 22, 1821.

THE experiments on the aëriform compounds of charcoal and hydrogen, described in the following pages, are supplementary to a Memoir on the same class of bodies, which the Royal Society did me the honour to insert in their Transactions for 1808, as well as to other papers on the same subject, which have been published in Mr. NICHOLSON's Journal, and in the Memoirs of the Manchester Society. Of these essays, I beg leave to offer a very brief recapitulation, with the view merely of connecting them with what is to follow.

In the first of these essays (NICHOLSON's Journal, 8vo. June, 1805), I detailed a series of experiments on the gases obtained by the destructive distillation of wood, peat, pit-coal, oil, wax, &c. from which it appeared that the fitness of those gases for artificial illumination was greater, as they required for combustion a greater proportional volume of oxygen; and that the gases generated from different inflammable bodies, or from the same inflammable substance under different circumstances, are not so many distinct species, which under such a view of the subject would be almost infinite in number, but are mixtures of a few well known gases, chiefly of carburetted hydrogen with variable pro-

portions of olefiant, simple hydrogen, sulphuretted hydrogen, carbonic acid, carbonic oxide, and azotic gases; and that the elastic fluids obtained from coal, oil, &c. have probably, in addition to these, an inflammable vapour diffused through them when recent, which is not removed by passing them through water.* In the same paper I explained certain anomalies that appear in the experiments of the late Mr. CRUICKSHANK, of Woolwich, which are not at all chargeable as errors upon that excellent chemist, and could only be elucidated by farther investigation of the gases to which they relate. Of his labours it would be unjust, indeed, to speak in any terms but those of approbation, for they may fairly be considered as the foundation of most that is now known respecting this species of aëriform bodies. To Mr. DALTON, also, we are indebted for an accurate acquaintance with carburetted hydrogen gas, and for much information that is valuable in assisting us to judge of the composition of mixed combustible gases, by the phenomena and results of firing them with oxygen.†

In the second Memoir (Philosophical Transactions, 1808), I described a series of experiments on the gases obtained from several different varieties of pit-coal, and from the same kind of coal under different circumstances. Various species of that mineral were found to yield aëriform products, differing greatly in specific gravity, combustibility, and illuminating power; the cannel coal of Wigan, in Lancashire, being best adapted to the purpose, and the stone-coal of South Wales the least so. In decomposing any one species of coal, the

* NICHOLSON'S Journal, 8vo. XI. 72.

† New System of Chemical Philosop y, *passim*.

gaseous fluids were ascertained not to be of uniform quality throughout the process, but to vary greatly at different stages; the heavier and more combustible gases coming over first, and the lighter and less combustible afterwards. By subsequent experiments on the gases obtained from coal on the large scale of manufacture, it was found that a similar decline in the value of the products takes place, but not to the same extent, owing, probably, to the greater uniformity of temperature, which is attainable in large operations.*

On the practical conclusions, which it was the object of the last mentioned Essay to establish, I forbear to dwell, because they are unconnected with my present purpose, which is limited to the chemical constitution of these compound gases, and to the methods of separating them accurately from each other. The view of their nature and composition, which was taken in the first Essay, was opposed by those able philosophers, M. BERTHOLLET, and Dr. MURRAY, of Edinburgh, who both contended for greater latitude as to the proportions in which hydrogen and charcoal are capable of uniting, and considered these proportions indeed as subject to no limitation. The facts, however, which have since been multiplied in this, as well as in other departments of chemistry, tending to prove, that bodies capable of energetic combination unite in a few definite proportions only, leave little doubt that the same law holds good with respect to the compounds of hydrogen and charcoal. Not that it is meant that the known compounds of those elements are the only possible ones; for others will probably be discovered, which will still be found conformable to the general law, *that when one body combines with another in*

* Manchester Society's Memoirs, new Series, vol. III.

different proportions, the greater proportions are multiples of the less by an entire number.

A different view of the subject has lately been taken by the ingenious author of the Bakerian Lecture, published in the Philosophical Transactions for 1820. In that paper, Mr. BRANDE has endeavoured to prove, that the gas called light carburetted hydrogen, or simply carburetted hydrogen, or hydro-carburet, is not entitled to be considered as a distinct species; that the only aëriform compound of charcoal and hydrogen, which is with certainty known to exist, is the gas called olefiant, or bi-carburetted hydrogen; and that the gases evolved by heat from coal and oil, are in fact nothing more than mixtures of olefiant and simple hydrogen gases in various proportions.

In assuming, in the first Essay, the existence of light carburetted hydrogen as a definite compound, characterized by its requiring, for the complete combustion of each volume, two volumes of oxygen, and giving one volume of carbonic acid, I relied on the sole authority of Mr. DALTON; for the gas of marshes, though before known to be inflammable, had not been subjected to accurate examination by any other chemist. Mr. CRUICKSHANK, indeed, speaks of it as "pure hydro-carbonate;"* but since he classes it in that respect with the gas obtained by the destructive distillation of camphor, from which it differs essentially in composition, it is plain that he was not correctly acquainted with the properties of pure carburetted hydrogen. Previously to the second set of experiments, I satisfied myself by the careful analysis of a specimen of the gas from stagnant water, for which I

* NICHOLSON'S Journal, 4to. vol. V. p. 6.

was indebted to Mr. DALTON, that it really has the properties which have been ascribed to it by him as characteristic; and in 1807 I found precisely the same characters in the fire-damp of coal-mines.* Dr. THOMSON, also, from experiments in 1811,† on the gas from stagnant water, and Sir HUMPHRY DAVY,‡ from the analysis of the fire-damp in 1815, drew the same conclusions. It is in the power, indeed, of every chemist to investigate for himself the properties and composition of carburetted hydrogen gas, since it may easily be procured in considerable quantity, by stirring the bottom of almost any stagnant pool, especially if composed of clay. During the last summer, I obtained it from a source of this kind, which afforded it in such abundance, that several gallons might have been collected in a few minutes. This gas I submitted to repeated and most careful examination. It contained $\frac{1}{20}$th its volume of carbonic acid, but no sulphuretted hydrogen whatever, and no proportion of oxygen gas that could be discovered by attentively testing it with nitrous gas. The results of its combustion with oxygen gas, effected in a Volta's eudiometer in the usual manner, showed that it was contaminated with $\frac{1}{15}$th its volume of azotic gas. Apart, however, from this, the pure portion, in a great number of trials, required, as nearly as can be expected in experiments of this sort, two volumes of oxygen for combustion, and gave one volume of carbonic acid. Its specific gravity, taken on quantities procured at three several times, varied only from ·582 to ·586, the mean of which is ·584; and this, allowing for $\frac{1}{15}$th of azotic gas of specific gravity ·972,

* NICHOLSON's Journal, 8vo. XIX. 149.

† Mem. of the Wernerian Society, I. 506. ‡ Phil. Trans. 1816, p. 5.

gives ·556 for the specific gravity of pure carburetted hydrogen gas, a number which coincides almost exactly with that found by Dr. THOMSON.* Since, therefore, the same results have been obtained from the examination of gases similarly collected at distant times and places, there appears to me no reason for refusing to consider carburetted hydrogen gas as a true chemical compound, characterized by perfect uniformity of properties and composition. At the temperature of 60° Fahrenheit, and under 30 inches pressure, 100 cubical inches must weigh 16·95 grains, and be composed (taking the weight of 100 cubic inches of carbonic acid at 46·5 grains, and the charcoal in 100 grains of that acid at 27·8 grains) of

	Grains.	Grains.	Grains.
Charcoal . .	12·69	74·87	100
Hydrogen . .	4·26	25·13	33·41
	16·95	100.	133·41

And olefiant gas (giving twice its volume of oxygen by combustion, and weighing 29·64 grains for 100 cubical inches†) must be constituted of

	Grains.	Grains.	Grains.
Charcoal . .	25·38	85·63	100
Hydrogen . .	4·26	14·37	16·71
	29·64	100.	116·71

And as 16·7 is to 100, so very nearly is 1 to 6, which

* Annals of Philosophy, Vol. XVI. p. 252.

† I adopt this result of Dr. THOMSON from its near coincidence with that of an experiment of my own, on the specific gravity of olefiant gas, published in the Phil. Trans. 1808, p. 293.

last number is the weight of the atom of charcoal, as deduced from the constitution of olefiant gas. It is true, that this determination a little exceeds that which is derived from the composition of carbonic acid (viz. 5·63), the atom of oxygen being taken at 7·5. But if 8 be the true number for oxygen, which now seems to be most probable both from experiment and analogy, we shall then find an exact coincidence between the relative weight of the atom of charcoal, as deduced from olefiant gas, and as determined from carbonic acid. Perhaps the true specific gravity of hydrogen gas, on which depend the relative weights of the atoms of hydrogen and oxygen, may be fully as correctly ascertained from the composition of carburetted hydrogen, as by direct attempts to weigh so light a fluid. Now, as the hydrogen in 100 cubic inches of hydro-carburet weighs only 4·26 grains, and is equivalent to 200 cubic inches of hydrogen gas, we have 2·13 grains for the weight of 100 cubic inches of hydrogen gas, from which may be deduced ·0698 for its specific gravity, that of air being 1. And if the specific gravity of oxygen gas be 1·111, it will be found that the two volumes of hydrogen, required to saturate one volume of oxygen gas, have as nearly as possible the relative weight of 1 to 8.

Were any additional argument necessary to establish the existence of carburetted hydrogen as a distinct species, it might be derived from the action of water on that gas, which, besides being absorbable in a constant proportion, admits of being expelled again by the application of heat, not otherwise changed than in having acquired a small quantity of those gases which are always present in water, and of

which it is impossible to deprive it even by long continued boiling.

The process, by which carburetted hydrogen gas is evolved in natural operations, is no doubt the decomposition of water, and admits of being explained on the atomic theory of Mr. DALTON, by supposing two atoms of charcoal to act at once on two atoms of water. One atom of charcoal attracts the two atoms of hydrogen, forming carburetted hydrogen gas, and the other atom of charcoal unites with two atoms of oxygen, constituting carbonic acid. This is illustrated by the annexed figure, in which two atoms of charcoal C.C. are represented as interposed between two atoms of water, each consisting of an atom of hydrogen and an atom of oxygen. Dividing the diagram vertically into three parts, we have the original substances; and separating it horizontally, we obtain the two new compounds. This theoretical view of the subject is confirmed by the fact, that the carburetted hydrogen, formed at the bottom of stagnant pools, is never accompanied by carbonic oxide, but always by carbonic acid, the full quantity of which is prevented from appearing, in consequence of the absorption of a great part of it by the mass of water, under which the changes are taking place.

Being provided with such an abundant supply of carburetted hydrogen, I availed myself of it to examine the mutual action of that gas and chlorine on each other, principally with a view to ascertain, how far reliance may be placed on the latter as an instrument in the analysis of mixed combustible gases. This is a part of the subject that was first investigated, though with a different view, by Mr. CRUICK-

SHANK.* He observed that a mixture of chlorine with hydrogen, carburetted hydrogen, or carbonic oxide in certain proportions, kept in a bottle entirely filled with the mixture, and furnished with an air-tight stopper, did not exhibit any immediate action, but that in twenty-four hours, on withdrawing the stopper, the fluid immediately rushed in, and filled most of the space originally occupied by the gases. But he was not aware of the influence of light on these changes, which was discovered about the same time by GAY LUSSAC† and by DALTON.‡ It does not, however, appear to have been ascertained by either of them, whether the complete exclusion of light prevents any degree of action of chlorine and carburetted hydrogen on each other. I mixed, therefore, those two gases in different proportions in well stopped vials, which were completely filled with the mixture, and covered by opake cases. When the stoppers were removed under water, at various intervals after the mixture, from a few minutes to 89 days, no diminution whatever of volume was found to have taken place; and after having removed the chlorine by liquid potash, the carburetted hydrogen gas gave the usual products of carbonic acid, and consumed the usual proportion of oxygen. Mixtures also of hydrogen and chlorine, and of carburetted hydrogen and chlorine, standing over water in graduated tubes, which were shaded by opake covers, sustained no loss of bulk, except what arose from the absorption of chlorine by the water, the combustible gas remaining wholly unaltered. It may be

* NICHOLSON's Journal, 4to. V. 202.

† Mem. de la Soc. d'Arcueil. II. 349.

‡ New System of Chemical Philosophy, p. 300.

considered, therefore, as quite essential to the mutual agency of these gases, that they should be subjected to the influence of light. But it is not necessary that the direct rays of the sun should fall on the mixture, the light of a dull and cloudy day being fully adequate to the effect. On a day of this sort, I filled several stoppered vials, graduated into hundredths of a cubic inch, with a mixture of 80 volumes of carburetted hydrogen with from 80 to 90 of chlorine, and uncovering them all at the same moment, exposed them to the feeble light which was then abroad. By exposure of one of the vials during half a minute, no diminution of volume was found to have been effected; another vial, opened under water when one minute had elapsed, showed an absorption of five parts; a third in two minutes had lost 15 parts; a fourth in four minutes 25 parts; and a fifth, opened in five minutes, contained only 50 volumes out of the original 110.

The products, resulting from the contact of carburetted hydrogen and chlorine, under circumstances favourable to their mutual action, have been described by Mr. CRUICKSHANK, with whose experience on this point my own entirely agrees When rather more than four volumes of chlorine are kept in mixture with one volume of gas from stagnant water, the products are muriatic acid gas, and a volume of carbonic acid equivalent to that of the pure carburetted hydrogen; and this, whether the mixture be exposed to direct or indirect solar light; the only difference being that the less intense the light, the more slowly is the effect produced. When less than four volumes of chlorine are employed, the residue consists of muriatic and carbonic acids, carbonic oxide,

and undecomposed carburetted hydrogen, the proportions of the two last increasing as, within certain limits, we reduce the relative quantity of chlorine. These changes were ascertained, both by Dr. DAVY and the late Dr. MURRAY,* to depend on the presence of moisture, which is unavoidably introduced in the common mode of operating; for when the gases, first perfectly dried, were mixed in an exhausted glass vessel, and exposed even to the direct rays of the sun, no mutual action was found to ensue. In the theory of these changes there is, it must be confessed, a little uncertainty. Does the chlorine, it may be asked, act simultaneously on the hydrogen of water, and on that of the combustible gas; or does it decompose water only? The former view of the subject appears to me most probable, because, if the chlorine acted on water only, free hydrogen would be evolved from that portion of the hydrocarburet which abandons its charcoal to the oxygen of the water; which is not consistent with experience. When it is required to form carbonic acid, four volumes of chlorine must be used for the decomposition of each volume of carburetted hydrogen. In this case, two atoms of chlorine unite with the two atoms of hydrogen existing in the combustible gas, and the two other atoms of chlorine with the two atoms of hydrogen from the water. But to convert carburetted hydrogen into carbonic oxide, three atoms of chlorine are sufficient, two of which are employed, as in the first case, and the third is expended in saturating the hydrogen of one atom of water, which supplies to the charcoal an atom of oxygen for the formation of carbonic oxide. Calculating in the same

* NICHOLSON's Journal, xxviii. 143, and 201.

manner, we shall find, also, that three atoms of chlorine are adequate to convert one atom of carbonic oxide into carbonic acid.

The facts which have been stated sufficiently prove, that chlorine cannot be employed as a means of correctly analyzing mixtures of olefiant gas, either with hydrogen or with carburetted hydrogen, if light be admitted, even though of feeble intensity, and for the short interval during which such an experiment may be expected to continue: and they explain that uncertainty as to the results of analyses of mixed gases made in this way, which was first remarked by Mr. FARADAY* and subsequently by myself.† Chlorine becomes, however, a most useful agent in separating olefiant gas from such mixtures, provided light be entirely excluded during its operation, as I have found by subjecting to its action, mixtures of those gases with known proportions of olefiant gas. In these analytical experiments, I admitted into a graduated tube standing over water, a volume of chlorine exceeding by about one half what was known to be sufficient, and noted its bulk when actually in the tube, which was immediately shaded by an opake cover. A measured quantity of the mixture was then passed up, and in about ten minutes the outer cover was cautiously lifted, till the surface of the water appeared. The diminution of volume thus ascertained, divided by 2, was found to give pretty correctly the quantity of olefiant gas known to be contained in the mixture. But the greatest precision was attained by waiting 15 or 20 minutes, and then quickly washing the remaining gas with dilute solution of potash, in order to remove the excess of chlorine.

* Journal of Science, &c. vi. 358. † Manchester Memoirs, new Series, vol. iii.

From the volume of the residuary gas, it was necessary to deduct the amount of impurity previously ascertained to exist in the chlorine; and the remainder, taken from the volume of mixed gases which had been operated on, showed how much olefiant gas had been condensed by the chlorine. When very narrow tubes were employed, and the column of gases mixed with chlorine was of considerable length, a longer continuance of the experiment was found necessary, and the gases were suffered to remain in contact during an hour or more. In this way it was ascertained, that olefiant gas may be accurately separated by chlorine from hydrogen, carburetted hydrogen, or carbonic oxide gases, or from mixtures of two or more of those gases, which are left quite unchanged in volume and in chemical properties, when light has been carefully excluded from the mixture.

This property of chlorine is the foundation of a fresh analysis, to which I have thought it expedient to submit the gases from coal and oil, in order to decide what aëriform fluids remain after the separation of that portion which is condensible by chlorine;—whether the residue consists, as I have heretofore maintained, of carburetted hydrogen chiefly, with variable proportions of hydrogen and carbonic oxide; or whether, according to the new view of the subject, it consists of hydrogen gas only.

In the experiments made for this purpose, I operated generally on from 60 to 80 cubic inches of oil gas or coal gas, assaying a small specimen first, as a guide to the quantity of chlorine which it would be necessary to employ. The volume of chlorine thus found to be requisite, and about half as much more, was passed into an air receiver standing over water, and

completely shaded by an opake cover which was fitted over it. The oil or coal gas was then added by degrees, if much condensation was expected, because in that case a considerable increase of temperature would have been produced by the sudden admixture of large quantities; or at once, if only a moderate action had been indicated by the previous assay. The mixture was allowed to stand, completely guarded from the light, during 30 or 40 minutes, or even longer, and the residue was expeditiously washed with liquid potash, and a small portion again assayed, to ascertain that the action of the chlorine was complete. The specific gravity of the washed gas was then carefully taken, that of the entire gas having been previously determined: and the results of its combustion with oxygen examined, and compared with those of the gas in its original state.

Experiments on the gas from oil.

In obtaining this gas at different times, I used the same kind of whale oil, which had been heated a little below its boiling point during two hours, in order to deprive it of water. The oil was admitted by drops into an ignited iron tube filled with fragments of broken crucibles, and no difference, that I am aware of, existed in the circumstances under which the decomposition was effected, except that the degree of heat was purposely lowered in the latter processes, till that temperature was attained, which was barely adequate to the production of gas. The oil gas procured from London, I obtained through the kindness of Mr. Richard Phillips. It had been prepared from cod oil, at the manufactory of Messrs. John and Philip Taylor, and having been conveyed to Manchester in bottles accurately stoppered and tied over with a double

fold of bladder, it was found not to have acquired any admixture with atmospheric air. The results are contained in the following table, in which the expression *entire gas* is applied to the gas precisely as it came over, except that the carbonic acid had been removed by liquid potash, applied in the smallest quantity and with the least agitation that were adequate to the effect.

TABLE I. *Containing the results of experiments on the gas obtained from whale oil.*

Entire Gas.					Residue left by chlorine.		
			100 vols.			100 vols.	
No. of Experiment.	Sp. Gr.	100 vols. lose by chlorine.	take oxyg.	give carb. ac.	Sp. Gr.	Take Oxyg.	Give Carb. ac.
1	.464	6	116	61	.4107	54	46
2	.590	19	178	100	.4400	108	58
3	.758	22.5	220	130	.6160	145	85
4(London)	.906	38	260	158	.6060	152	91

From the foregoing table it appears, that the gas obtained at different times from oil of the same quality, is far from being of uniform composition, and that great differences, as to its specific gravity and chemical properties, are occasioned by the temperature at which it is produced. So far as my experience goes, no temperature short of ignition is sufficient for the decomposition of oil into permanent combustible gases; but the lower the heat that is employed, provided it be adequate to the effect, the heavier and more combustible is the gas, and the better suited to artificial illumination.

From the experiments which I published in 1805, and which were made on a single specimen of oil gas, I was led to consider it as constituted of one volume of olefiant gas

with seven volumes of mixed gases, of which the greatest part was carburetted hydrogen. Mr. DALTON has since favoured me with a specimen of oil gas prepared by himself, which contained in 100 parts, 40 of a gas condensible by chlorine; and it appears from the table that oil gas, manufactured on the large scale, may contain in 100 parts, 38 parts of a gas similarly characterized.* It is not improbable indeed that by a temperature carefully regulated, the whole of the aëriform fluids may be obtained from oil, of such quality as to be entirely condensible by chlorine; and from the great superiority of the light which such a gas would afford, and the reduction that might be effected in the capacity of the gasometers, the discovery of a mode of producing it in this state, would be an important practical improvement.

The inferences respecting the nature of the gas from oil, I reserve till after the account of the experiments on coal gas, as the same remarks, with some slight modifications, will apply to both cases.

Experiments on the gas from coal.

The numerous experiments and observations on the gas from coal, which I have already published, appear to me to preclude the necessity of going much into the subject on this occasion. What I have lately had in view, has been to render the analysis of this gas more complete, by a careful exami-

* Since this Paper was written, I have received from Mr. PHILLIPS a second specimen of oil gas prepared by Messrs. TAYLOR. It contains in every 100 volumes, 42 or 43 parts of gas condensible by chlorine; but in other respects very nearly agrees, (making allowance for the greater proportion of that ingredient) with the gas described in the text.

nation of that portion of it which remains after the action of chlorine. The gas, submitted to these recent experiments, was prepared from Wigan cannel, at the manufactory of Messrs. PHILIPS and LEE. It was collected from an opening in a pipe between the retort and the tar-pit, generally about an hour after the commencement of the distillation, except in the instance of the gas No. 4, which was taken five hours, and No. 5, which was taken ten hours, from that period. Before using it, the carbonic acid and sulphuretted hydrogen, which were always present in the early products, were separated by careful ablution with liquid potash. As the gas No. 5, was not at all diminished by chlorine, it was obviously unnecessary to examine it in any but its entire state.

TABLE II. *Containing the results of experiments on the gas obtained from coal.*

	Entire Gas.				Gas left by Chlorine.		
		100 volumes.				100 volumes.	
Experiment.	Sp. Gr.	Take oxyg.	Give car. ac.	Loss by Chlorine.	Sp. Gr.	Take oxyg.	Give car. acid.
1	.650	217	128	13	.575	178	92
2	.620	194	106	12	.527	160	82
3	.630	196	108	12	.535	148	80½
4	.500	166	93	7	.450	140	75
5	.345	78	30	0			

Inferences respecting the composition of that part of the gases from coal and from oil, which is not condensible by the action of chlorine.

The analytical experiments, which I have described on the action of chlorine on artificial mixtures of olefiant with hydrogen and carburetted hydrogen gases, afford no room for

doubt that by that agent the quantity of olefiant gas in any mixture of these gases may be accurately determined. We are not, however, acquainted with any chemical agent, either liquid or aëriform, which, from a mixture of hydrogen, carburetted hydrogen, and carbonic oxide, is capable of separating one of those gases, leaving the others in their original state and quantity.* The only method at present known of determining the composition of such a mixture is by firing it with oxygen gas, and, from the phenomena and results of the process, deducing the proportion of its ingredients. In drawing conclusions of this kind, it is necessary to have distinctly in view the properties of those gases in their separate state. The following Table contains an abstract of their leading characters, which will be found very useful in such investigations. Though not strictly necessary, I have included olefiant gas, in order to render the Table more complete.

TABLE III. *Exhibiting the characteristic properties of different combustible gases.*

Names of Gases.	Sp. Gr. Air 1000	100 Volumes require Oxygen.	Total.	Diminished by Firing.	Carb. Acid produced.
Olefiant Gas	·970	300	400	200 = ½	200
Carburetted Hydrogen	·556	200	300	200 = ⅔	100
Hydrogen Gas	·069	50	150	150 = ⅔	0
Carbonic Oxide	·972	50	150	50 = ⅓	100

* I have not found that chlorine can be employed with any success in analyzing such mixtures; for when placed in contact with two or more of those gases, and exposed to light, it does not act upon one exclusively, but upon all that compose the mixture.

As an illustration of the method of investigating the proportions of mixtures of the three last gases, we may take the instance of a mixed gas, free from olefiant gas, of specific gravity ·534, of which 100 volumes consume 110 of oxygen, and afford 70 of carbonic acid, the diminution of the whole 210 after firing being 140 volumes. Now it must be obvious from inspection of the Table, that the 70 parts of carbonic acid cannot all have resulted from the combustion of carburetted hydrogen, since, for the saturation of 70 measures of that gas, 140 of oxygen would have been required, whereas only 110 have been expended. We may therefore safely infer the presence of carbonic oxide, a gas which, by combustion, gives its own volume of carbonic acid, with the expenditure of only half its volume of oxygen. The specific gravity of the specimen being lower than that of carburetted hydrogen, indicates also an admixture of simple hydrogen gas; and of this the proportion must necessarily be considerable, to countervail the weight of the heavy carbonic oxide. The following proportions of the three gases will be found to coincide with the properties of the mixture.

	Consume Ox.	Give Carb. Ac.	Dimin. by Firing.
40 vols. of carb. hydrogen	80	40	80
30 ——— carb. oxide	15	30	15
30 ——— hydrogen gas	15	0	45
100	110	70	140

No reliance, however, can be placed on the accuracy of such estimates, unless the specific gravity of the specimen agrees with that of the hypothetical mixture, as deduced from the proportion of its ingredients. But when this coincidence takes place, we have all the evidence, which the

subject at present admits, of the nature of the mixture; and as this agreement between experiment and calculation was found to take place very nearly, in all the instances comprehended in the two following Tables, we may consider the numbers composing them, as expressing, with sufficient exactness, the relative proportion of different gases in the residues of oil and coal gas left by the action of chlorine.

TABLE IV. *Showing the composition of* 100 *volumes of the gas remaining after the action of chlorine on oil gas.*

Exp.	Azote.	Carb. Hydr.	Carb. Oxide	Hydr. Gas.	Total.
1	7	30	15	48	100
2	5	40	15	40	100
3	5	65	20	10	100
4	5	75	15	5	100

TABLE V. *Showing the composition of* 100 *volumes of the gas remaining after the action of chlorine on coal gas.*

Exp.	Azote.	Carb. Hydr.	Carb. Oxide	Hydr. Gas.	Total.
1	1.5	94.5	4	0	100
2	6	82	2	10	100
3	2	66	14	18	100
4	5	60	12	23	100
5	10	20	10	60	100

It appears from the two foregoing Tables, that the portion of oil gas and coal gas, which is not condensible by chlorine, is in every case a mixed gas, consisting in most instances of carburetted hydrogen, carbonic oxide, and hydrogen, with a little azote, part of which may be traced to the impurity of the chlorine. In the best specimens of oil gas, the carbonic

oxide is in greater proportion than in the best kinds of gas from coal, and the carburetted hydrogen is most abundant in the latter gas. This, however, is more than compensated, so far as their illuminating power is concerned, by the greater richness of the aëriform products of oil in that denser species of gas, which is separable by chlorine. The proportion of hydrogen, both in oil gas and coal gas, appears to increase as they are formed at a higher temperature, and is always greatest in the latter portions of the gas from coal. But no instance has ever occurred to me of a gas obtained from oil or from coal, which, after the action of chlorine upon it with the exclusion of light, presented a residuum at all approaching to simple hydrogen gas; nor do I believe that such a gas can be generated under any circumstances of temperature, by which the decomposition of coal or of oil is capable of being effected.

Inferences respecting the composition of that part of the gas from coal and oil, which is condensed by contact with chlorine.

When a given volume of a mixture of olefiant and carburetted hydrogen gases is fired with oxygen, and an equal volume of the same mixture is first deprived of olefiant gas by the action of chlorine, and then fired with oxygen, it must necessarily happen that the excess of oxygen spent in the first combustion, above that consumed in the second, will be three times the volume of the olefiant gas, and that the excess of carbonic acid formed in the first experiment above that generated in the second, will be double the volume of the olefiant gas. A remarkable anomaly however, was, during the last summer, observed by Mr. DALTON in the

results of the combustion of a quantity of gas, which he had himself prepared from oil. One volume was found to consume three volumes of oxygen, and to yield little short of two volumes of carbonic acid, in those respects agreeing nearly with olefiant gas; but when mingled with more than the requisite proportion of chlorine, it was not, as olefiant gas would have been, entirely condensed, but suffered a diminution of only four tenths of its bulk, the remaining six tenths, after being freed from the redundant chlorine, agreeing in its properties with carburetted hydrogen. For example, 10 volumes of this gas (containing four of gas condensible by chlorine and six of carburetted hydrogen) consumed 30 volumes of oxygen, and gave 18 of carbonic acid. But of the oxygen, 12 volumes are due to the six of carburetted hydrogen, leaving 18 volumes for the combustion of the four volumes of gas condensible by chlorine, which is in the proportion of $4\frac{1}{2}$ to 1. Of the 18 volumes of carbonic acid, also, six may be traced to the combustion of the carburetted hydrogen, leaving 12 volumes as the product of four of the condensible gas, or in the proportion of 3 to 1. The portion of gas, condensed by the action of chlorine presents, therefore, decided differences from olefiant gas, in requiring not three only, but $4\frac{1}{2}$ volumes of oxygen for combustion, and in affording 3, instead of 2 volumes of carbonic acid. Nearly the same relation of the oxygen consumed, and carbonic acid produced, to that part of the gases from coal and oil which is condensible by chlorine, existed also not only in other experiments of Mr. DALTON, but in all those which I have myself made. The proportions I have found to vary in different cases from $4\frac{1}{2}$ to 5 volumes of

oxygen, and from 2¼ to 3 volumes of carbonic acid for each volume of the condensible gas.

On comparing also the specific gravity of the gases from coal and oil, as ascertained by experiment, with that which ought to result from mixtures of the residue left by chlorine, with such a proportion of olefiant gas as is deducible from analysis, I have invariably found, that the real specific gravity has considerably exceeded the estimated. For instance, the London oil gas was composed of 38 volumes of a gas condensible by chlorine, and 62 volumes of mixed gases not characterized by that property, and having the specific gravity .606. But 62 volumes of gas of specific gravity .606, mixed with 38 volumes of olefiant gas of specific gravity .970, should give a mixture of the specific gravity .754, instead of .906, which was the actual specific gravity of the entire oil gas. It will be found on calculation that the 38 volumes of gas, in order to make up the real specific gravity of the oil gas, must have had the specific gravity of 1.4 very nearly. This is the highest number that is deducible from my experiments for the specific gravity of that portion of oil gas or coal gas, which is condensed by the action of chlorine. In other instances, it varied from that number down to 1.2, but in every case its weight surpassed that of common air.

It is evident from these facts that the aëriform ingredient of oil gas and coal gas, which is reducible to a liquid form by chlorine, is not identical with the olefiant gas obtained by the action of sulphuric acid on alcohol, but considerably exceeds that gas in specific gravity and combustibility. Two views may be taken of its nature; for it may either be a gas *sui generis*, hitherto unknown, and constituted of hydrogen and

charcoal in different proportions from those composing any known compound of those elements;—or it may be merely the vapour of a highly volatile oil, mingled in various proportions with olefiant gas, carburetted hydrogen, and the other combustible gases. Of these two opinions, Mr. DALTON is inclined to the first, considering it as supported by the fact that oil gas, or coal gas, may be passed through water, without being deprived of the ingredient in question; and that this anomalous elastic fluid is absorbed by agitation with water, and again expelled by heat or other gases, unchanged as to its chemical properties, as we have both satisfied ourselves by repeated experiments. On the other hand, I have found that hydrogen gas, by remaining several days in narrow tubes in contact with fluid naphtha, acquires the property of being affected by chlorine precisely as if it were mixed with a small proportion of olefiant gas; and I am informed by Dr. HOPE, that oil gas, when forcibly compressed in GORDON's portable gas lamp, deposits a portion of a highly volatile essential oil. The smell also of the liquid which is condensed on the inner surface of a glass receiver, in which oil gas or coal gas has been mixed with chlorine, denotes the presence of chloric ether, evidently however mingled with the odour of some other fluid, which seems to me to bear most resemblance to that of spirit of turpentine. This part of the subject is well worthy of farther investigation; but having devoted to the enquiry all the leisure which I am now able to command, I must remain satisfied at present with such conclusions as are safely deducible from the foregoing investigation. These may be briefly recapitulated as follows:

1. That carburetted hydrogen gas must still be considered

as a distinct species, requiring for the perfect combustion of each volume two volumes of oxygen, and affording one volume of carbonic acid; and that if olefiant gas be considered as constituted of one atom of charcoal united with one atom of hydrogen, carburetted hydrogen must consist of one atom of charcoal in combination with two atoms of hydrogen.

2. That there is a marked distinction between the action of chlorine on olefiant gas, (which, in certain proportions, is entirely independent of the presence of light, and is attended with the speedy condensation of the two gases into chloric ether), and its relation to hydrogen, carburetted hydrogen, and carbonic oxide gases, on all which it is inefficient, provided light be perfectly excluded from the mixture.

3. That since chlorine, under these circumstances, condenses olefiant gas without acting on the other three gases, it may be employed in the correct separation of the former from one or more of the three latter.

4. That the gases evolved by heat from coal and from oil, though extremely uncertain as to the proportions of their ingredients, consist essentially of carburetted hydrogen, with variable proportions of hydrogen and carbonic oxide; and that they owe, moreover, much of their illuminating power to an elastic fluid, which resembles olefiant gas in the property of being speedily condensed by chlorine.

5. That the portion of oil gas and coal gas, which chlorine thus converts into a liquid form, does not precisely agree with olefiant gas in its other properties; but requires, for the combustion of each volume, nearly two volumes of oxygen more than are sufficient for saturating one volume of olefiant gas, and affords one additional volume of carbonic acid. It is

probably, therefore, either a mixture of olefiant gas with a heavier and more combustible gas or vapour, or a new gas *sui generis*, consisting of hydrogen and charcoal, in proportions that remain to be determined.

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MDCCCXXI.

METEOROLOGICAL JOURNAL
for January, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sir's Therm.		Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	26	43	29,42	23		NW	1	Cloudy and foggy.
	2	0	28	42	29,49	29		NW	1	Cloudy.
2	8	0	31	40	29,60	24		S	1	Fine.
	2	0	37	39	29,52	38		SW	1	Cloudy.
3	8	0	34	40	29,52	33		SW	1	Cloudy, some snow.
	2	0	33	48	29,86	37		NW	1	Fine.
4	8	0	27	41	30,07	27		SW	1	Fog.
	2	0	32	57	30,05	31			1	Fine, rather hazy.
5	8	0	19	35	30,14	20		W	1	Foggy.
	2	0	30	44	30,16	34		W	1	Cloudy.
6	8	0	28	37	30,11	26		W	1	Cloudy.
	2	0	35	48	30,12	37		SSW	1	Cloudy and hazy.
7	8	0	34	41	30,22	33		SE	1	Cloudy.
	2	0	30	47	30,39	35		E	1	Cloudy.
8	8	0	27	42	30,52	27		N	1	Fine, rather hazy.
	2	0	29½	45	30,55	32		N	1	Cloudy.
9	8	0	24	39	30,68	25		N	1	Cloudy, snow in the night
	2	0	33	39	30,63	34		N	1	Cloudy.
10	8	0	23	37	30,51	23		S	1	Cloudy, snow in the night
	2	0	27	45	30,45	29		S	1	Cloudy, some snow.
11	8	0	26	38	29,98	23		S	1	Cloudy, snow in the night
	2	0	32	47	29,73	31		S	1	Snow.
12	8	0	37	39	29,97	35		E	1	Cloudy.
	2	0	26	46	30,13	29		E	1	Cloudy.
13	8	0	21	31	30,14	20			1	Thick fog.
	2	0	28	42	30,13	39		E	1	Cloudy and hazy.
14	8	0	26	39	30,19	33		E	1	Cloudy.
	2	0	27	40	30,16	38		E	1	Cloudy.
15	8	0	25	34	29,87	23½			1	Cloudy.
	2	0	23	39	29,73	37		E	1	Cloudy.
16	8	0	25	37	29,68	23				Cloudy and foggy.
	2	0	28	31	29,71	34		NW		Cloudy.

Rain this Month 0,444 Inches.

METEOROLOGICAL JOURNAL

for January, 1820.

1820	Time.	Therm. without.	Therm. within.	Barom.	Six's Therm.		Winds.		Weather.
	H. M.	°	°	Inches.			Points.	Str.	
Jan. 17	8 0	31	35	29,65	26		W	1	Cloudy and foggy.
	2 0	33	44	29,56	35		W	1	Cloudy.
18	8 0	29	36	29,37	28½			1	Cloudy, snow in the night
	2 0	32	45	29,34	33		E	1	Cloudy.
19	8 0	47	43	28,84	33		E	1	Cloudy, rain in the night.
	2 0	37	47	28,94	47		W	1	Cloudy.
20	8 0	36	43	29,52	32		W	1	Cloudy.
	2 0	33	57	29,41	35		W	1	Cloudy.
21	8 0	33	45	29,15	31½		W	1	Cloudy.
	2 0	36	50	29,31	37		SSE	1	Cloudy.
22	8 0	29	43	30,03	27		SE	1	Cloudy.
	2 0	32	48	30,12	34½		ESE	1	Cloudy and hazy.
23	8 0	32	41	30,11	27		ESE	1	Cloudy.
	2 0	39	43	30,07	39		SbyW	1	Fine.
24	8 0	38	42	29,84	35		S	2,3	Cloudy.
	2 0	43	45	29,74	43		SW	1,2	Rain.
25	8 0	43	45	29,86	38		NE	1	Rain.
	2 0	42	53	29,66	46		NNE	1	Rain.
26	8 0	44	48	29,48	40		W	1	Cloudy.
	2 0	48	53	29,62	46		W	1	Cloudy.
27	8 0	48	50	29,58	45		W	1,2	Fine.
	2 0	48	57	29,51	53		W	2	Cloudy.
28	8 0	43	52	29,55	43		N	1	Cloudy.
	2 0	45	51	29,78	47		SE	1	Fine.
29	8 0	40	52	30,17	40		N	1	Cloudy.
	2 0	33	54	30,17	36		N	1	Fine.
30	8 0	41	51	30,07	39		N	1	Cloudy.
	2 0	44	56	30,05	46		N	1	Cloudy.
31	8 0	42	50	30,00	41		S	1,2	Fine.
	2 0	46	56	30,00	51		SW	1	Cloudy.

Rain this Month 0,444 Inches.

METEOROLOGICAL JOURNAL

for February, 1820

1820	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Feb. 1	8 0	38	51	29,99	37		SbyE	1	Fine, rather hazy.
	2 0	45	59	29,90	51		E	1	Fine.
2	8 0	34	50	29,83	34		E	1	Foggy.
	2 0	40	56	29,80	42		E	1	Hazy and cloudy.
3	8 0	35	50	29,96	34		N	1	Hazy.
	2 0	36	54	29,99	37		N	1	Hazy.
4	8 0	34	48	30,04	34		W	1	Hazy and cloudy.
	2 0	38	51	30,04	38		SbyE	1	Cloudy.
5	8 0	39	47	29,90	35		S	2	Cloudy.
	2 0	44	52	29,91	43		S	1	Rain.
6	8 0	43	49	29,82	42		S	1	Cloudy.
	2 0	46	49	29,91	47		S	1	Cloudy.
7	8 0	47	49	30,11	47		SW	1	Cloudy.
	2 0	48	52	30,12	49		S	1	Cloudy.
8	8 0	44	52	30,14	43		W	1	Cloudy.
	2 0	45	55	30,15	45		SW	1	Cloudy.
9	8 0	42	50	30,05	41		S	1.2	Fine.
	2 0	50	57	29,92	52		S	1	Fine.
10	8 0	45	54	29,85	41		W	1	Cloudy.
	2 0	47	52	29,96	50		SW	1	Fine.
11	8 0	37	51	30,12	35		W	1	Cloudy.
	2 0	48	55	30,02			S	1	Cloudy.
12	8 0	41	52	29,85	40		S	1	Cloudy and hazy.
	2 0	43	54	29,94	44		SW	1	Cloudy.
13	8 0	40	51	30,03	39		S	1	Cloudy.
	2 0	44	50	30,00	42		S	1	Fine.
14	8 0	37	49	30,18	36		NE	1	Cloudy.
	2 0	48	54	30,23	48		N	1	Cloudy.
15	8 0	36	47	30,32	34		S	1	Cloudy.
	2 0	41	54	30,31	43		NbyE	1	Fine.

Rain this Month 1,435 Inches.

METEOROLOGICAL JOURNAL

for February, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sia's Therm.	Winds. Points	Str.	Weather.
Feb. 16	8	0	28	40	30,27	30	N	1	Cloudy.
	2	0	40	42	30,24	38	N	1	Fine.
17	8	0	29	43	30,11	29	NW	1	Foggy.
	2	0	41	51	30,08	43	NW	1	Fine.
18	8	0	27	46	30,09	26	NNE	1	Cloudy and hazy.
	2	0	35	51	30,08	35	SE	1.2	Fine.
19	8	0	33	46	30,10	32	N	1	Cloudy.
	2	0	34	49	30,08	36	SSE	1.2	Cloudy.
20	8	0	32	45	29,91	31	WSW	1	Snow.
	2	0	33	43	29,86	33	W	1	Cloudy.
21	8	0	30	42	29,83	30	S	1	Cloudy.
	2	0	36	50	29,83	38	N	1	Rain.
22	8	0	36	46	29,81	35	N	1	Rain.
	2	0	48	48	29,59	37	S	1	Cloudy.
23	8	0	46	48	29,67	45	SSE	1	Cloudy.
	2	0	47	52	29,52	49	SSE	1	Rain.
24	8	0	38	48	29,45	37	SW	1	Cloudy.
	2	0	39	53	29,38	39	N	1	Rain
25	8	0	36	49	29,44	36	NbyE	2.3	Rain.
	2	0	39	52	29,52	39	N	1	Cloudy.
26	8	0	35	48	29,93	35	E	1	Cloudy.
	2	0	35	54	30,02	37	N	1	Cloudy.
27	8	0	32	46	30,11	31	N	1	Cloudy.
	2	0	39	47	30,09	43	N	1	Fine.
28	8	0	32	43	30,00	30	E	1	Fine.
	2	0	40	48	29,94	48	E	1	Cloudy.
29	8	0	28	44	29,87	28	SW	1	Cloudy.
	2	0	42	49	29,74	48	W	1	Fine.

Rain this Month 1,425 Inches.

METEOROLOGICAL JOURNAL
for March, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sir's Therm.		Winds. Points.	Scr.	Weather.
Mar. 1	7	0	39	47	29,50	37		N	1	Cloudy.
	2	0	45	53	29,57	55		N	1	Cloudy.
2	7	0	34	46	28,95	34		N	2.3	Snow.
	2	0	41	55	29,17	42		E	2	Fine.
3	7	0	31	44	29,70	29		N	1.2	Fine.
	2	0	43	54	29,84	43		N	1	Fine.
4	7	0	31	45	29,99	30		N	1	Fine.
	2	0	38	50	29,99	40		SE	1	Fine.
5	7	0	28	43	30,16	31		NE	1	Fine.
	2	0	41	43	30,16	42		NE	1	Fine.
6	7	0	31	43	30,16	30		N	1	Cloudy.
	2	0	36	47	30,15	59		NE	1	Cloudy.
7	7	0	31	43	30,17	28		N	1	Cloudy.
	2	0	34	50	30,12	35		W	1	Rain.
8	7	0	34	44	30,24	34		NbyE	1	Cloudy.
	2	0	41	49	30,25	45		E	1	Cloudy.
9	7	0	33	44	30,18	30		W	1	Fine.
	2	0	44	51	30,06	47		W	1	Cloudy.
10	7	0	35	47	29,87	33		S	1	Cloudy.
	2	0	42	52	29,71	46		SSE	1	Fine.
11	7	0	35	47	29,64	33		E	1	Cloudy and hazy.
	2	0	45	57	29,58	56		E	1	Fine.
12	7	0	34	49	29,47	33		E	1	Cloudy.
	2	0	47	53	29,41	57		E	1	Fine.
13	7	0	40	48	29,57	38		NW	1.2	Cloudy.
	2	0	48	55	29,72	50		NW	1	Fine.
14	7	0	43	49	30,07	38		W	2	Cloudy.
	2	0	52	57	30,15	53		SSW	1	Cloudy.
15	7	0	49	53	30,23	48		NW	1	Cloudy.
	2	0	58	57	30,25	61		N	1	Cloudy.
16	7	0	46	54	30,36	46		E	1	Cloudy.
	2	0	48	58	30,36	51		E	1	Fine.

Rain this Month 1,325 Inches.

METEOROLOGICAL JOURNAL
for March, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Rain this Month 1,325 Inches.	Winds. Points.	Str.	Weather.
Mar. 17	7	0	40	54	30,27	39		SW	1	Cloudy, and very hazy.
	2	0	50	60	30,25	50		N	1	Fine.
18	7	0	38	52	30,34	37		SE	1	Fine.
	2	0	54	60	30,33	55		E	1	Fine. [night.
19	7	0	41	52	30,25	40		N	1	Cloudy, some rain in the
	2	0	43	55	30,22	47		E	1	Cloudy.
20	7	0	38	49	30,24	38		N	1	Cloudy and hazy.
	2	0	44	56	30,22	46		SW	1	Fine.
21	7	0	36	49	30,16	35		W	1	Cloudy and hazy.
	2	0	49	55	30,02	50		W	1	Cloudy.
22	7	0	44	51	29,76	39		N	1	Fine.
	2	0	50	63	29,80	52		NW	1	Fine.
23	7	0	45	53	29,36	44		W	1.2	Fine, rain in the night.
	2	0	48	60	29,11	53		WNW	2.3	Cloudy.
24	7	0	43	54	29,07	40		W	1	Fine.
	2	0	48	60	28,93	54		S	1	Fine.
25	7	0	38	51	29,17	37		NW	1	Fine.
	2	0	46	63	29,36	46		S	1	Fine.
26	7	0	36	51	29,76	33		SSW	1	Cloudy.
	2	0	43	53	29,58	43		SSW	1	Cloudy, rain at 1 P. M.
27	7	0	37	52	29,77	38		W	1	Rain.
	2	0	51	62	29,80	52		SbyW	1	Cloudy.
28	7	0	47	53	29,95	45		W	1.2	Fine.
	2	0	57	63	30,01	60		SW	1	Fine.
29	7	0	50	56	30,17	48		SW	1	Fine.
	2	0	62	64	30,07	61		SW	1	Fine.
30	7	0	49	54	29,94	48		SW	1	Fine.
	2	0	64	63	29,96	64		N	1	Fine.
31	7	0	45	55	30,01	41		SW	1	Cloudy.
	2	0	60	67	29,98	66		S	1	Fine.

METEOROLOGICAL JOURNAL

for April, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sit'n Therm.	Winds. Points.	Winds. Str.	Weather.	
Apr. 1	7	0	42	54	30,01	41	W	2	Cloudy.	
	2	0	55	58	29,97	60	W	1	Cloudy.	
2	7	0	52	56	30,06	47	W	1	Cloudy.	
	2	0	56	59	30,06	64	W	1	Fine.	
3	7	0	53	58	30,16	52	NE	1	Cloudy.	
	2	0	58	61	30,17	59	E	1	Cloudy.	
4	7	0	47	55	29,98	42	SE	1.2	Fine and clear.	
	2	0	64	64	29,88	66	E	1	Fine.	
5	7	0	48	57	29,80	45	NE	1	Hazy thick weather.	
	2	0	62	66	29,71	60	S	1	Fine.	
6	7	0	48	57	29,40	45	SE	1	Rain.	
	2	0	54	60	29,38	50	W	1	Rain.	
7	7	0	37	53	29,43	36	W	1	Fine.	
	2	0	55	61	29,47	55	SW	1	Cloudy.	
8	7	0	41	53	29,38	37	S	1.2	Fine.	
	2	0	50	61	29,27	52	SbyE	1.2	Cloudy.	
9	7	0	46	53	29,23	45	SSW	1	Cloudy.	
	2	0	53	56	29,36	54	NW	1	Cloudy.	
10	7	0	44	53	29,49	38	SSE	2	Cloudy.	
	2	0	47	60	29,38	48	S	2	Rain.	
11	7	0	47	53	29,44	46	SW	1	Cloudy.	
	2	0	53	62	29,54	54	SW	1	Fine.	
12	7	0	48	55	29,71	47	SW	1	Cloudy and hazy.	
	2	0	56	61	29,79	56	S	1	Cloudy.	
13	7	0	49	55	29,86	48	WSW	1	Cloudy and hazy.	
	2	0	50	61	29,82	52	N	1	Rain.	
14	7	0	49	57	29,59	47	SSW	1	Cloudy and hazy.	
	2	0	50	61	29,59	52	WNW	1	Rain.	
15	7	0	45	55	29,75	42	WNW	1	Fine.	
	2	0	51	62	29,81	54	WNW	1	Fine.	

Rain this Month 1,514 Inches.

METEOROLOGICAL JOURNAL

for April, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Apl. 16	7	0	47	55	30,04	40		WNW	1	Fine.
	2	0	58	60	30,06	58		WNW	1	Fine.
17	7	0	55	56	30,19	44		W	1	Fair.
	2	0	62	62	30,18	67		N	1	Fair.
18	7	0	51	58	30,16	58		N	1	Cloudy and hazy.
	2	0	60	65	30,13	64		SSE	1	Fine.
19	7	0	56	59	30,13	50		N	1	Fine.
	2	0	63	64	30,09	66		N	1	Fine.
20	7	0	53	59	30,11	49		NW	1	Fine.
	2	0	67	65	30,18	67		N	1	Cloudy.
21	7	0	51	59	30,27	48		NW	1	Fine.
	2	0	61	67	30,25	65		N	1	Fine.
22	7	0	53	59	30,33	48		E	1	Fine.
	2	0	59	63	30,34	60		E	1	Fine.
23	7	0	51	57	30,46	45		ESE	1	Fine.
	2	0	60	63	30,47	60		ESE	1	Fine.
24	7	0	50	58	30,50	44		NE	1	Fine.
	2	0	62	66	30,47	65		E	1.2	Fine.
25	7	0	48	56	30,42	44		N	1	Cloudy.
	2	0	60	64	30,31	61		N	1	Fine.
26	7	0	52	55	29,97	45		WNW	1	Fine.
	2	0	57	60	29,78	68		WSW	1	Fine.
27	7	0	45	55	29,67	45		N	2	Cloudy, rain in the night.
	2	0	45	56	29,81	49		NNW	1	Cloudy.
28	7	0	43	55	29,97	41		SSW	1	Cloudy.
	2	0	51	60	30,02	55		W	1	Fine.
29	7	0	48	55	30,08	40		W	1	Fine.
	2	0	55	59	30,08	63		W	1	Cloudy.
30	7	0	44	53	30,15	49		NW	1	Rain.
	2	0	56	58	30,19	69		WNW	1	Fine.

Rain this Month 1,514 Inches.

METEOROLOGICAL JOURNAL

for May, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
May 1	7	0	47	52	30,28	42		SbW	1	Fine,
	2	0	54	57	30,29	67		SSW	1	Fine.
2	7	0	47	50	30,25	45		W	1	Cloudy.
	2	0	56	58	30,15	60		E	1	Cloudy.
3	7	0	45	56	30,14	45		ESE	1	Cloudy and hazy.
	2	0	49	56	30,10	50		E	1	Cloudy and hazy.
4	7	0	45	56	29,98	43½		E	1	Cloudy and hazy.
	2	0	53	59	29,88	57		E	1	Cloudy.
5	7	0	43	54	29,85	39		NE	1	Cloudy.
	2	0	52	60	29,84	56		N	1	Fine,
6	7	0	45	54	29,88	40		W	1	Fine.
	2	0	55	60	29,81	60		W	1	Fine.
7	7	0	50	52	29,58	48		WSW	1	Cloudy.
	2	0	55	57	29,54	60		WSW	1	Fine.
8	7	0	54	56	29,68	51		SW	1	Cloudy, dull weather.
	2	0	60	58	29,66	66		WSW	1	Cloudy.
9	7	0	53	60	29,62	51		S	1	Cloudy, rain in the night.
	2	0	60	64	29,66	68		WSW	1	Fine.
10	7	0	55	59	29,78	53		S	2	Fine.
	2	0	63	62	29,83	64		S	2	Cloudy.
11	7	0	54	59	29,92	52		S	2,3	Fine, rather cloudy.
	2	0	61	66	29,95	61		S	1	Fine.
12	7	0	54	60	30,00	49		W	1	Cloudy.
	2	0	62	63	30,03	65		W	1	Fine.
13	7	0	53	60	29,96	49		E	1	Hazy.
	2	0	60	60	29,87	60		SSW	1	Cloudy.
14	7	0	55	58	29,86	51		SSW	1	Fine.
	2	0	59	61	29,81	63		SSW	1	Fine.
15	7	0	52	58	29,79	48		S	1	Fair.
	2	0	63	64	29,79	65		S	1	Fine.
16	7	0	53	60	29,68	51		W	1	Cloudy, rain in the night.
	2	0	58	63	29,66	63		SSW	1	Fine.

Rain this Month 1,393 Inches.

METEOROLOGICAL JOURNAL
for May, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
May 17	7	0	53	59	29,74	49		SW	1.2	Cloudy.
	2	0	61	65	29,74	60		SW	1	Fine.
18	7	0	48	60	29,33	47		SW	1.2	Rain.
	2	0	54	62	29,17	56		S	3	Rain.
19	7	0	54	59	29,64	47		SW	1.2	Fine.
	2	0	60	62	29,79	70		WSW	1	Cloudy.
20	7	0	53	60	30,13	49		SW	1	Cloudy.
	2	0	60	62	30,21	65		SW	1	Cloudy.
21	7	0	54	60	30,22	49		SW	1	Fine.
	2	0	59	63	30,31	68		WSW	1	Fine.
22	7	0	54	60	30,20	52		E	1	Fine.
	2	0	68	68	30,14	68		E	1	Fine.
23	7	0	59	62	29,99	55		E	1	Fine.
	2	0	67	71	29,94	75		E	1	Fine.
24	7	0	62	64	29,71	56		E	1	Fine.
	2	0	67	67	29,76	69		W	1	Fair.
25	7	0	54	61	29,74	51		S	2	Rain.
	2	0	56	63	29,74	60		W	1.2	Rain.
26	7	0	52	59	29,80	47		W	1	Cloudy.
	2	0	56	62	29,78	62		S	2	Cloudy.
27	7	0	53	61	29,66	53		W	1	Rain.
	2	0	57	62	29,62	64		W	1	Fine.
28	7	0	53	59	29,68	50		NW	1	Cloudy.
	2	0	55	60	29,57	58		W	1	Rain.
29	7	0	53	57	29,35	43		NW	1	Cloudy.
	2	0	54	58	29,33	60		SW	1	Rain.
30	7	0	49	57	29,31	44		W	1	Fine.
	2	0	60	60	29,33	60		SSW	1	Fine.
31	7	0	52	56	29,34	47		W	1	Fine.
	2	0	57	61	29,39	59		N	1	Fine.

Rain this Month 1,593 Inches.

METEOROLOGICAL JOURNAL
for June, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
June 1	7	0	50	55	29.54	45		W	2	Fine.
	2	0	59	61	29.54	59		WNW	1	Fine.
2	7	0	52	57	29.55	49		SW	1	Fine.
	2	0	57	60	29.57	59		W	1	Rain.
3	7	0	50	57	29.71	46		WbN	1	Cloudy.
	2	0	60	60	29.73	60		NW	1	Cloudy.
4	7	0	50	56	29.86	49		NW	1	Rain.
	2	0	56	57	29.88	57		NW	1	Cloudy.
5	7	0	51	56	29.95	45		N	1	Fine.
	2	0	63	59	29.94	63		W	1	Cloudy.
6	7	0	52	57	29.83	51		W	1	Cloudy.
	2	0	58	57	29.97	59		W	1	Cloudy.
7	7	0	50	56	30.06	47		W	1	Fine.
	2	0	61	59	29.95	63		N	1	Cloudy.
8	7	0	54	57	29.94	54		N	1	Cloudy.
	2	0	64	59	29.92	64		WbN	1	Cloudy.
9	7	0	55	59	29.80	54		W	1	Rain.
	2	0	61	60	29.73	63		NW	1	Cloudy.
10	7	0	50	57	29.72	45		W bN	1	Fine.
	2	0	57	58	29.72	57		W bN	1	Fine.
11	7	0	51	57	29.61	45		WNW	1	Fine.
	2	0	52	56	29.55	53		WNW	1	Cloudy.
12	7	0	52	56	29.69	49		NW	1	Rain.
	2	0	60	60	29.77	65		E	1	Cloudy.
13	7	0	50	56	29.85	45		NW	1	Fine.
	2	0	58	59	29.85	63		NW	1	Cloudy, some rain
14	7	0	52	59	30.06	49		SE	1	Cloudy.
	2	0	57	58	30.05	59		SE	1	Fine.
15	7	0	52	57	29.81	49		NW	1	Rain.
	2	0	57	58	29.89	63		NW	1	Cloudy.

Rain this Month 2,331 Inches.

METEOROLOGICAL JOURNAL
for June, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
June 16	7	0	53	56	29.94	48		N	1	Cloudy.
	2	0	59	58	29.94	60		NW	1	Cloudy.
17	7	0	55	57	29.92	53		NNW	1	Cloudy.
	2	0	63	60	29.95	66		W	1	Cloudy.
18	7	0	57	57	30.06	52		W	1	Fine.
	2	0	64	62	30.01	64		NNE	1	Fine.
19	7	0	57	59	29.70	52		N	1	Cloudy, rain in the night.
	2	0	63	63	29.72	65		NW	1	Fine.
20	7	0	53	59	29.64	51		W	1	Cloudy.
	2	0	60	60	29.75	66		NW	1	Rain.
21	7	0	54	59	29.85	50		NW	1	Fine.
	2	0	64	61	29.90	65		N	1	Cloudy.
22	7	0	56	59	30.02	52		NW	1	Cloudy.
	2	0	67	62	30.02	70		W	1	Cloudy.
23	7	0	58	60	30.11	54		W	1	Fine.
	2	0	60	61	30.13	65		W	1	Fine.
24	7	0	61	63	30.18	58		SW	1	Fine.
	2	0	72	73	30.21	76		SSW	1	Fine.
25	7	0	67	67	30.32	63		N	1	Fine.
	2	0	75	72	30.33	80		SSW	1	Fair.
26	7	0	70	70	30.38	65		W	1	Fine.
	2	0	84	77	30.37	84		NE	1	Fine.
27	7	0	71	70	30.37	70		E	1	Fine.
	2	0	79	76	30.37	85		WSW	1	Fine.
28	7	0	70	72	30.31	66		S	1	Fine.
	2	0	82	79	30.25	84		NW	1	Fine.
29	7	0	62	69	30.18	60		E	1	Cloudy.
	2	0	72	76	30.09	85		E	1	Fine.
30	7	0	62	69	29.94	58		E	1	Cloudy and hazy.
	2	0	67	67	29.98	69		E	1	Cloudy.

Rain this Month 2,331 Inches.

METEOROLOGICAL JOURNAL

for July, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six'h Therm.		Winds. Points.	Winds. Str.	Weather.
July 1	7	0	59	64	30,18	58		N	1	Fine, rather cloudy.
	2	0	64	65	30,22	66		N	1	Fine.
2	7	0	56	64	30,17	50		W	1	Fine.
	2	0	60	63	30,05	65		W	1	Rain.
3	7	0	54	62	29,96	51		NW	1	Cloudy, rather hazy.
	2	0	62	64	29,91	64		W	1	Cloudy.
4	7	0	57	61	29,92	54		NNW	1	Cloudy.
	2	0	60	61	29,98	63		E	1	Cloudy.
5	7	0	55	61	30,07	54		N	1	Fine.
	2	0	60	60	30,09	61		NbE	1	Cloudy.
6	7	0	54	60	30,11	50		NE	1	Cloudy.
	2	0	63	62	30,12	64		NbE	1	Cloudy.
7	7	0	57	61	30,21	54		NbE	1	Cloudy.
	2	0	63	63	30,12	65		N	1	Fine.
8	7	0	56	61	30,15	55		NE	1	Cloudy, rather hazy.
	2	0	60	60	30,10	64		N	1	Cloudy.
9	7	0	56	60	30,16	54		NbE	1	Cloudy.
	2	0	65	60	30,14	64		NE	1	Cloudy.
10	7	0	58	60	30,12	58		E	1	Cloudy.
	2	0	66	65	30,08	68		E	1	Fine, rather cloudy.
11	7	0	56	61	30,06	53		EbS	1	Fine.
	2	0	66	64	30,02	67		ESE	1	Fine.
12	7	0	53	60	29,97	52		E	1	Fine.
	2	0	65	65	29,88	68		E	1	Cloudy.
13	7	0	59	61	29,86	56		EbS	1	Cloudy.
	2	0	62	61	29,82	65		ESE	1	Cloudy.
14	7	0	59	61	29,82	55		NNE	1	Cloudy.
	2	0	65	62	29,84	68		NE	1	Fine.
15	7	0	62	63	29,90	60		SW	1	Cloudy.
	2	0	76	65	29,93	73		WSW	1	Fine.
16	7	0	60	63	29,95	57		E	1	Fine. [der storm at 11 A.M.
	2	0	72	67	29,90	74		SE	1	Fine, rather cloudy, a thun-

Rain this Month 2,807 Inches.

METEOROLOGICAL JOURNAL

for July, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
July 17	7	0	62	66	29,64	60		E	1	Rain.
	2	0	68	65	29,46	70		W	1	Cloudy, rain from 11 till 2.
18	7	0	55	63	29,39	55		S	1	Rains hard.
	2	0	67	65	29,35	69		WSW	1	Cloudy.
19	7	0	56	63	29,42	55		W	1	Cloudy, rather hazy.
	2	0	68	66	29,52	70		W	1	Cloudy.
20	7	0	58	63	29,65	56		W	1	Fine.
	2	0	69	70	29,71	70		WNW	1	Fine.
21	7	0	61	64	29,84	59		SW	1	Cloudy.
	2	0	70	66	29,91	71		W	1	Fine.
22	7	0	60	62	29,93	56		WSW	1	Fine.
	2	0	62	64	29,91	70		WSW	1	Cloudy.
23	7	0	57	63	29,89	59		WbN	1	Cloudy.
	2	0	65	63	29,94	66		NW	1	Cloudy.
24	7	0	58	63	29,95	55		W	1	Cloudy.
	2	0	69	66	29,95	70		NW	1	Fine.
25	7	0	62	65	29,86	60		W	1	Fine.
	2	0	67	67	29,84	70		W	1	Fine.
26	7	0	56	63	29,98	54		W	1	Fine.
	2	0	69	67	30,04	70		WNW	1	Fine.
27	7	0	62	65	30,04	60		W	1	Cloudy.
	2	0	72	72	30,02	72		WSW	1	Fine.
28	7	0	62	65	30,00	58		W	1	Fine.
	2	0	71	69	30,02	72		SW	1	Fine.
29	7	0	64	67	30,06	62		E	1	Cloudy.
	2	0	73	72	30,07	75		SSW	1	Fine.
30	7	0	63	67	30,06	61		SW	1	Fine.
	2	0	75	71	30,02	77		SE	1	Cloudy.
31	7	0	64	69	29,83	62		E	1	Fine.
	2	0	75	77	29,83	80		W	1	Fine.

Rain this Month 2,807 Inches.

METEOROLOGICAL JOURNAL
for August, 1820.

1820	Time. H.	M	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Aug. 1	7	0	53	71	29,78	46		W	1	Cloudy.
	2	0	73	70	29,83	75		SSW	1	Cloudy.
2	7	0	57	60	30,04	56		E	1	Fine.
	2	0	71	70	30,08	75		WSW	1	Fine.
3	7	0	64	68	29,98	60		S	1,2	Cloudy.
	2	0	71	69	29,89	75		S	1,2	Cloudy.
4	7	0	64	68	29,72	63		S	1	Cloudy, rain in the night.
	2	0	72	71	29,74	75		SWbS	1	Fine.
5	7	0	59	67	29,79	57		W	1	Fine.
	2	0	69	70	29,82	72		WSW	1	Cloudy. [in the night
6	7	0	57	65	29,72	56		S	2	Rain, much rain and wind
	2	0	68	67	29,58	70		WSW	1,2	Cloudy.
7	7	0	60	64	29,68	57		W	1	Cloudy.
	2	0	69	69	29,87	70		WbN	1	Cloudy.
8	7	0	54	63	29,96	58		WSW	1	Cloudy.
	2	0	69	65	29,99	70		WSW	1	Cloudy.
9	7	0	57½	64	29,91	60		ESE	1	Fine.
	2	0	69	71	30,04	70		WNW	1	Fine.
10	7	0	59	65	30,25	55		W	1	Cloudy.
	2	0	71	72	30,26	72		S	1	Fine.
11	7	0	61	66	30,25	59		W	1	Hazy.
	2	0	72	73	30,22	72		W	1	Fine.
12	7	0	60	66	30,16	62		W	1	Cloudy.
	2	0	71	72	30,15	77		W	1	Fine.
13	7	0	61	67	30,11	59		N	1	Fine, rather cloudy.
	2	0	70	72	30,06	71		NW	1	Fine.
14	7	0	61	67	29,98	59		SbW	1	Fine.
	2	0	72	75	29,94	75		SSW	1	Fine.
15	7	0	60	67	29,88	57		W	1	Fine.
	2	0	73	73	29,82	75		WSW	1	Fine.
16	7	0	64	69	29,78	62		W	1	Cloudy.
	2	0	72	71	29,81	74		W	1,2	Cloudy.

Rain this Month 1,315 Inches.

METEOROLOGICAL JOURNAL

for August, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Sir's Therm.		Winds. Points.	Str.	Weather.
Aug. 17	7	0	66	68	29,79	63		SW	2	Cloudy.
	2	0	71	71	29,76	73		W	2	Fair.
18	7	0	61	68	29,83	59		N	1	Hazy and cloudy.
	2	0	73	69	29,81	74		WSW	1	Fine.
19	7	0	59	66	29,72	57		E	1	Rather cloudy.
	2	0	68	66	29,71	70		WSW	1	Cloudy.
20	7	0	53	64	29,84	50		W	1	Fine.
	2	0	64	72	29,85	67		SSW	1	Cloudy.
21	7	0	58	63	29,82	56		N	1	Rain.
	2	0	58	64½	29,77	65		ESE	1	Rain.
22	7	0	54	62	29,76	53		N	2	Cloudy, rain in the night.
	2	0	59	62	29,79	60		WSW	2	Cloudy.
23	7	0	52	59	29,99	50		N	1	Fine, rather hazy.
	2	0	60	61	30,09	62		N	1	Cloudy.
24	7	0	56	60	30,15	52		E	1	Fine.
	2	0	63	64	30,08	66		WSW	1	Fine.
25	7	0	57	60	29,91	52		S	1.2	Cloudy.
	2	0	65	62	29,80	66		SW	1.2	Cloudy.
26	7	0	57	61	29,67	55		SW	2	Fine.
	2	0	64	65	29,66	68		W	1	Cloudy.
27	7	0	53	61	29,66	51		W	1	Fine.
	2	0	64	64	29,72	66		WSW	1	Rain.
28	7	0	54	62	29,48	53		WbN	1	Fine and clear.
	2	0	60	64	29,51	65		SW	1	Fine.
29	7	0	52	60	29,61	50		W	1	Fine.
	2	0	64	66	29,69	66		ESE	1	Fair.
30	7	0	51	61	29,86	49		WbN	1	Fine, but rather hazy.
	2	0	61	64	29,99	64		SSW	1	Fine.
31	7	0	53	61	30,02	51		EbS	1	Cloudy and hazy.
	2	0	61	65	30,04	63		NE	1	Fair.

Rain this Month 1,315 Inches.

METEOROLOGICAL JOURNAL
for September, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Sep. 1	7	0	53	60	30,07	51		NE	1	Fine, rather hazy.
	2	0	64	63	30,05	65		NE	1	Fine.
2	7	0	54	60	30,03	52		N	1	Fine, rather hazy.
	2	0	63	64	30,03	64		ESE	1	Fine.
3	7	0	57	61	30,08	55		N	1	Cloudy.
	2	0	64	64	30,11	64		SSE	1	Fine, rather hazy.
4	7	0	50	60	30,14	48		SW	1	Cloudy and hazy.
	2	0	65	64	30,14	66		N	1	Cloudy.
5	7	0	52	60	30,10	50		NbyE	1	Hazy.
	2	0	55	65	30,07	56		ENE	1	Fine.
6	7	0	53	61	30,03	51		NE	1	Cloudy, rather hazy.
	2	0	63	68	30,03	64		E	1	Fine.
7	7	0	54	61	30,07	51		E	1	Hazy.
	2	0	62	66	30,07	64		SE	1	Fine.
8	7	0	57	63	30,17	55		W	1	Cloudy and hazy.
	2	0	67	66	30,25	67		N	1	Fine.
9	7	0	53	63	30,34	52		E	1	Hazy.
	2	0	65	69	30,35	68		WNW	1	Fine.
10	7	0	54	63	30,30	52		SW	1	Cloudy,
	2	0	66½	69	30,27	67		WNW	1	Fair.
11	7	0	57	64	30,26	56		N	1	Hazy thick weather.
	2	0	69½	70	30,25	72		ESE	1	Cloudy.
12	7	0	61	66	30,21	59		E	1	Hazy.
	2	0	70	73	30,22	73		ESE	1	Fine.
13	7	0	58	66	30,17	56		E	1	Hazy.
	2	0	69	74	30,13	70		E	1	Fine.
14	7	0	59	66	29,96	56		S	1	Fine.
	2	0	71	72	29,88	72		SSE	1	Fine, rather hazy.
15	7	0	60	67	29,69	58		SW	1.2	Fine.
	2	0	63	67	29,76	71		WSW	1	Fine, rather hazy.

Rain this Month 2,058 Inches.

METEOROLOGICAL JOURNAL

for September, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Winds. Points.	Str.	Weather.
Sep. 16	7	0	55	62	29,94	51	W	1	Fine.
	2	0	64	64	29,96	65	W	1	Fine.
17	7	0	57	63	29,97	54	W	1	Fine.
	2	0	62	63	29,91	65	W	1	Fine. [night.
18	7	0	54	62	29,53	53	N	1	Rain; a heavy rain in the
	2	0	56	61	29,59	66	N	1.2	Cloudy.
19	7	0	46	67	29,80	43	WNW	1	Fine.
	2	0	54	63	29,94	56	WbN	1	Fair.
20	7	0	42	57	29,98	40	W	1	Fine.
	2	0	55	59	29,75	58	SW	1	Rain.
21	7	0	44	56	29,44	44	W	1	Fine.
	2	0	54	59	29,41	57	NW	1.2	Cloudy.
22	7	0	47	55	29,64	43	N	1	Cloudy, rather hazy.
	2	0	56	58	29,82	59	WNW	1	Cloudy.
23	7	0	56	57	29,96	47	SW	1	Cloudy.
	2	0	64	60	29,97	65	W	1	Fine.
24	7	0	57	59	29,75	56	SSW	2	Rain.
	2	0	65	61	29,58	66	W	1	Fine.
25	7	0	51	57	29,66	49	W	1	Fine.
	2	0	53	62	29,65	64	NW	1	Fine.
26	7	0	48	56	29,83	44	N	1	Cloudy.
	2	0	53	58	29,99	56	NNE	1	Cloudy.
27	7	0	41	53	30,12	38	W	1	Cloudy.
	2	0	53	56	30,04	55	W	1	Rain.
28	7	0	48	54	30,07	41	SSW	1	Cloudy.
	2	0	59	58	30,03	60	W	1	Cloudy.
29	7	0	53	57	29,95	48	NE	1	Rain.
	2	0	57	56	30,11	61	SSE	1	Cloudy.
30	7	0	48	56	30,16	46	E	1	Cloudy and hazy.
	2	0	61	58	30,05	62	S	1	Cloudy.

Rain this Month 2,058 Inches.

METEOROLOGICAL JOURNAL
for October, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Oct. 1	8	0	52	57	30,07	48		NNE	1	Cloudy.
	2	0	57	61	30,21	62		N	1	Cloudy.
2	8	0	49	56	30,36	46½		W	1	Fine, rather hazy.
	2	0	57	59	30,42	58		NNW	1	Cloudy.
3	8	0	50	56	30,49	46		NbE	1	Fine, rather hazy.
	2	0	56	60	30,53	58		NNE	1	Fine.
4	8	0	46	55	30,52	43		NE	1	Fine.
	2	0	56	57	30,44	58		ESE	1	Fine.
5	8	0	51	56	30,34	46		N	1	Fine, rather hazy.
	2	0	56	61	30,27	58		NE	1	Fine.
6	8	0	51	56	30,23	48		NEbN	1	Cloudy.
	2	0	58	60	30,19	59		E	1	Fine.
7	8	0	49	57	30,16	48		N	1	Fine, rather hazy.
	2	0	58	61	30,14	60		ESE	1	Cloudy.
8	8	0	49	56	30,14	48		EbN	1	Cloudy.
	2	0	58	58	30,17	60		E	1	Cloudy.
9	8	0	50	56	30,20	49		N	1	Cloudy.
	2	0	55	57	30,19	58		NNW	1	Cloudy.
10	8	0	47	55	30,13	47		N	1	Cloudy.
	2	0	64	56	30,07	65		SW	1	Cloudy.
11	8	0	47	55	29,99	46		N	1	Fine.
	2	0	52	53	29,95	54		N	1	Cloudy.
12	8	0	45	49	30,01	44		N	1	Fine.
	2	0	50	56	30,04	52		N	1	Cloudy.
13	8	0	39	51	29,99	39		W	1	Fine, rather hazy.
	2	0	50	53	29,95	52		E	1	Cloudy.
14	8	0	47	52	29,75	40		SE	1	Cloudy.
	2	0	52	49	29,40	54		SbE	1	Cloudy. [hard in the night.
15	8	0	58	56	29,03	50		S	3	Stormy ; rained & blowed
	2	0	57	58	29,01	62		S	1	Cloudy.
16	8	0	47	54	29,15	46		W	1	Cloudy.
	2	0	51	55	29,13	56		SW	1	Fine.

Rain this Month 1,582 Inches.

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1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Oct. 17	8	0	48	53	28,84	45		S	2	Rain.
	2	0	52	57	28,84	55		W	2	Fine.
18	8	0	43	54	29.93	43		W	1	Cloudy, rather hazy.
	2	0	52	57	29.98	54		W	1	Rain.
19	8	0	46	54	30,26	46		W	1	Cloudy.
	2	0	53	55	29,31	55		SSW	1	Cloudy.
20	8	0	43	53	28,86	43		WSW	1.2	Fine.
	2	0	49	55	29.92	54		WSW	1	Cloudy.
21	8	0	40	52	29.33	41		W	1	Hazy.
	2	0	51	55	29.49	53		N	1	Fine. [rain.
22	8	0	40	52	29,09	40		S	3	A violent gale of wind and
	2	0	50	54	28,71	52		S	1.2	Cloudy.
23	8	0	46	52	28,97	45		N	1	Cloudy.
	2	0	51	57	29,13	53		NNW	1	Cloudy.
24	8	0	45	53	28,88	45		SW	1	Fine.
	2	0	48	56	28,69	52		WSW	1	Cloudy.
25	8	0	44	55	28,96	42		WSW	1	Cloudy and hazy.
	2	0	51	58	29,20	53			1	Cloudy.
26	8	0	42	53	29,38	41		S	1	Cloudy.
	2	0	49	57	29,03	52		SSW	1	Cloudy.
27	8	0	47	54	28,96	41		W	1	Fine.
	2	0	52	57	29,11	57		W	1	Cloudy.
28	8	0	43	55	29,54	41		W	1	Fine.
	2	0	51	59	29,66	52		NW	1	Fine.
29	8	0	42	53	29,54	41		SbyE	1	Cloudy.
	2	0	46	53	29.27	52		SSE	1	Rain.
30	8	0	41	53	29,51	40		W	1	Cloudy.
	2	0	48	02	29,62	50		WSW	1	Cloudy.
31	8	0	44	55	29.49	41		WSW	1	Cloudy and hazy.
	2	0	49	58	29,35	50		ESE	1	Cloudy.

Rain this Month 1,582 Inches.

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for November, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Nov. 1	8	0	45	56	29.33	44		N	1	Cloudy and dark.
	2	0	47	58	29.46	49		N	1	Cloudy.
2	8	0	37	54	29.73	37		W	1	Fine.
	2	0	47	60	29.75	48		WNW	1	Fine.
3	8	0	34	46	29.82	34		W	1	Foggy.
	2	0	46	56	29.86	47		SW	1	Fine.
4	8	0	36	49	29.85	35		N	1	Foggy.
	2	0	47	55	29.86	49		NE	1	Fine.
5	8	0	31	49	29.93	31		W	1	Hazy.
	2	0	42	49	29.85	47		ESE	1	Rain.
6	8	0	47	51	29.69	31		W	1	Cloudy and hazy.
	2	0	52	57	29.74	53		W	1	Cloudy.
7	8	0	50	54	29.74	47		SW	1	Cloudy.
	2	0	55	58	29.74	56		S	1	Fine.
8	8	0	51	57	29.79	51		SE	1	Cloudy and hazy.
	2	0	54	57	29.81	57		ESE	1	Rain.
9	8	0	47	56	29.89	47		E	1	Cloudy.
	2	0	49	58	29.93	54		ESE	1	Cloudy and hazy.
10	8	0	44	57	29.97	44		N	1	Cloudy.
	2	0	48	58	30,10	52			1	Fine.
11	8	0	44	56	30,11	43		NbE	1	Cloudy.
	2	0	48	58	30,19	50		SSW	1	Cloudy.
12	8	0	39	53	30,13	39		NW	1	Cloudy.
	2	0	45	57	29.95	49			1	Cloudy.
13	8	0	39	51	29.57	38		NW	1	Cloudy.
	2	0	40	54	29,52	41			1	Cloudy.
14	8	0	35	51	29.76	35		N	1	Hazy.
	2	0	39	52	29.77	39		SSW	1	Cloudy.
15	8	0	35	49	29,87	38		NbE	1	Fine.
	2	0	40	52	29.91	43		NE	1	Fine.

Rain this Month 1,000 Inches.

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for November, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.	Winds. Points.	Winds. Str.	Weather.
Nov. 16	8	0	36	47	29,92	35	NbW	1	Fine, rather hazy.
	2	0	40	52	29,85	42	NbW	1	Cloudy and hazy.
17	8	0	35	50	29,67	32	SE	1	Cloudy and hazy.
	2	0	43	56	29,63	44		1	Rain.
18	8	0	36	51	29,80	35	NW	1	Hazy.
	2	0	45	55	29,86	47	WSW	1	Cloudy and hazy.
19	8	0	37	50	29,91	35		1	Foggy.
	2	0	45	50	29,91	47	WSW	1	Cloudy.
20	8	0	39	51	29,95	38	SbE	1	Fine.
	2	0	49	53	29,93	50	S	1.2	Cloudy.
21	8	0	48	52	29,88	47	SSW	1.2	Cloudy.
	2	0	49	57	29,83	51	SSW	1	Cloudy and hazy.
22	8	0	46	53	29,77	46	SbyE	1	Rain.
	2	0	49	58	29,74	50	SbyE	1	Rain.
23	8	0	44	54	29,57	44	E	1	Fine.
	2	0	47	61	29,50	50	NE	1	Cloudy.
24	8	0	40	57	29,68	39		1	Thick fog.
	2	0	48	59	29,70	49	NE	1	Cloudy.
25	8	0	43	55	29,56	40	E	1	Cloudy.
	2	0	47	57	29,56	48	ESE	1	Cloudy.
26	8	0	45	55	29,78	47	S	1	Fine.
	2	0	42	55	29,84	44	SSW	1	Cloudy.
27	8	0	42	53	29,89	42	E	1	Cloudy.
	2	0	48	57	29,90	52	E	1	Fine.
28	8	0	40	52	30,05	38	NE	1	Cloudy.
	2	0	43	57	30,11	47	SSE	1	Cloudy and hazy.
29	8	0	39	53	30,26	39	E	1	Cloudy.
	2	0	44	57	30,27	44	E	1	Cloudy.
30	8	0	42	52	30,23	42	NE	1	Cloudy.
	2	0	41	55	30,19	44	WNW	1	Cloudy.

Rain this Month 1,000 Inches.

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for December, 1820.

1820	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Winds. Str.	Weather.
Dec. 1	8	0	38	52	30,15	37		NW	1	Cloudy.
	2	0	42	51	30,12	45			1	Cloudy.
2	8	0	49	51	29,93	49		W	1	Cloudy.
	2	0	45	52	29,93	48		SSW	1	Cloudy, and hazy.
3	8	0	38	48	30,08	36		W	1	Hazy and small rain,
	2	0	46	51	30,02	47		WSW	1	Cloudy and hazy.
4	8	0	40	51	29,93	40		W	1	Cloudy.
	2	0	53	58	29,94	54		NE	1,2	Fine.
5	8	0	48	54	29,90	48		NE	1	Cloudy.
	2	0	52	55	29,85	54			1	Cloudy and hazy.
6	8	0	48	57	29,98	48		E	1	Rain.
	2	0	46	58	30,00	49			1	Rain.
7	8	0	50	56	30,01	46		W	1	Cloudy.
	2	0	55	58	30,14	57			1	Cloudy and hazy.
8	8	0	50	59	30,15	49		W	1	Cloudy.
	2	0	52	59	30,15	54		SW	1	Cloudy and hazy.
9	8	0	49	57	30,17	47		W	1	Cloudy.
	2	0	52	58	30,14	53		NNW	1	Cloudy and hazy.
10	8	0	49	55	30,01	48		SW	1,2	Cloudy.
	2	0	52	56	29,98	53		WNW	1	Cloudy—Rain.
11	8	0	50	55	29,84	49		W	2	Cloudy.
	2	0	53	58	29,88	54		W	1	Cloudy.
12	8	0	50	57	29,60	48		WSW	1	Rain.
	2	0	54	59	29,55	56		WNW	1	Cloudy.
13	8	0	48	59	29,49	47		S	1	Rain.
	2	0	38	57	29,48	54		N	1	Cloudy.
14	8	0	34	47	29,83	34		N	1	Fine.
	2	0	39	55	29,92	40		E	1	Fine.
15	8	0	36	52	29,95	35		E	1	Cloudy.
	2	0	33	55	29,92	39		SE	1	Fine.
16	8	0	30	58	29,70	29		E	1	Cloudy.
	2	0	35	57	29,56	36		SW	1	Cloudy, some snow.

Rain this Month 1,187 Inches.

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for December, 1820.

1820	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Six's Therm.		Winds. Points.	Str.	Weather.
Dec. 17	8	0	36	46	29,60	30		E	1	Thick fog.
	2	0	40	45	29,75	41		WSW	1	Cloudy.
18	8	0	41	47	30,02	36		E	1	Cloudy.
	2	0	46	50	30,11	47		SSE	1	Cloudy.
19	8	0	46	49	30,20	41		S	1.2	Rain.
	2	0	50	54	30,25	51		W	1	Rain.
20	8	0	41	50	30,34	41		S	1	Cloudy.
	2	0	48	52	30,29	50		WSW	1	Cloudy.
21	8	0	46	53	30,07	41		W	1	Cloudy.
	2	0	50	57	30,11	53		NW	1	Cloudy.
22	8	0	38	53	30,12	38		W	1	Fine.
	2	0	47	55	30,11	50			1	Cloudy.
23	8	0	40	52	29,97	38		N by E	1	Cloudy.
	2	0	44	53	29,92	46		N	1	Cloudy.
24	8	0	36	46	29,86	36		N	1	Cloudy.
	2	0	36	48	29,85	44		NNW	1	Cloudy.
25	8	0	32	45	29,84	32		NNE	1	Cloudy.
	2	0	32	49	29,83	36		SSE	1.2	Cloudy.
26	8	0	30	43	29,74	30		E	1	Cloudy.
	2	0	33	47	29,77	35		ESE	1.2	Cloudy and hazy.
27	8	0	31	43	29,86	30		E	1	Cloudy.
	2	0	33	57	29,88	35		E	1	Cloudy and hazy.
28	8	0	30	43	29,89	30		ESE	1	Fine.
	2	0	31	47	29,92	35		E	1	Cloudy.
29	8	0	27	41	29,95	27		E	1	Cloudy.
	2	0	34	44	29,93	28		ENE	1.2	Cloudy.
30	8	0	27	39	29,91	27		E	1	Cloudy.
	2	0	28	44	29,91	29		E	1	Cloudy.
31	8	0	25	38	29,92	25		N	1	Cloudy.
	2	0	29	39	29,87	30		E	1	Fair.

Rain this Month 1,187 Inches.

1820.	Thermometer without.			Thermometer within.			Barometer.*			Six's Thermometer.			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	48	19	33,5	57	31	44,4	30,68	28,34	29,87	53	20	34,1	0,444
February	50	27	39,1	59	40	49,6	30,32	28,38	29,95	52	26	39,1	1,425
March	64	28	43,1	67	43	52,9	30,36	28,93	29,88	66	28	43,8	1,325
April	67	37	52,2	67	53	58,6	30,50	29,23	29,92	69	36	52,2	1,514
May	63	45	55,1	71	50	59,9	30,31	29,17	29,65	70	39	55,5	1,393
June	84	50	59,8	79	55	61,4	30,38	29,54	29,93	85	45	59,4	2,331
July	75	53	62,5	77	60	64,2	30,22	29,35	29,93	80	50	62,5	2,807
August	73	51	62,5	75	59	66,2	30,26	29,48	29,98	77	46	61,0	1,315
September	71	41	57,0	74	53	62,0	30,35	29,41	29,98	73	38	56,9	2,058
October	64	40	49,5	61	49	55,5	30,53	28,71	29,67	65	39	49,8	1,582
November	55	31	43,4	61	46	54,2	30,27	29,33	29,84	56	31	43,9	1,000
December	55	25	41,3	59	38	51,2	30,34	29,49	29,94	57	25	41,9	1,187
Whole year			49,9			56,7			29,88			50,0	18,381

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle.

June 1819,	-	-	-	24.14.47 west.
June 1820,	-	-	-	24.11.44 west.
Dip. about	-	-	-	71.6